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ABSTRACT

This study is one of the "knowledge development" activities mounted in conjunction with research, evaluation, and development activities funded under the Youth Employment and Demonstration Projects Act of 1977 (YEDPA). It focuses on the impacts of Job Corps participation on a large group of participants after 18 months to two years. Significant benefits are documented, benefits which exceed the costs to society of the investment in Job Corps. Material covered in the study includes an overview of the Job Corps and the evaluation; overall impacts on employment and related activities; changes in family composition and employment-related impacts for females with children; comparison of benefits and cost; analysis of differential factors among participants and centers; analysis of program expenditures; and generalizability of the findings. (KC)

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YOUTH KNOWLEDGE DEVELOPMENT REPORT

PROGRAM EVALUATIONS
The Lasting Impacts of Job Corps Participation

May 1980

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YOUTH KNOWLEDGE DEVELOPMENT REPORT 3.4

THE LASTING IMPACTS OF JOB CORPS PARTICIPATION

Charles Mallar
Stuart Kerachsky
Craig Thornton
Michael Donihue
Carol Jones
David Long
Emmanuel Noggoh
Jennifer Schore

Mathematica Policy Research, Inc.

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OVERVIEW

Job Corps is the cornerstone of youth employment and training programs. Throughout its fifteen years of operation, the program has focused on the most disadvantaged youth, offering comprehensive services including vocational training, basic and advanced education, shelter, food, health care, recreation, counseling and allowances. The pricetag of this comprehensive approach is substantial--in excess of $12,000 per corpsmember year in fiscal 1980--making Job Corps the most expensive manpower program for youth.

Job Corps has been analyzed more than any other social welfare program over the years because of its cost but also because it is the most intensive investment in human resources, providing the best test of whether comprehensive services can alter the lives of severely disadvantaged youth. Studies have ranged in technical sophistication as well as in their conclusions. There is general agreement that Job Corps has positive impacts on participants, but disagreement whether the magnitude of benefits is sufficient to offset the high costs. Because Job Corps is so intensive, the benefits should be large enough to be measureable despite the lack of exactitude in assessment instruments. However, previous evaluations have not been reliable enough to reach definitive conclusions, either lacking adequate sample sizes or technical rigor.

In contrast, the following evaluation represents one of the most rigorous assessments ever mounted for a major employment and training program. Initiated by the Office of Policy, Evaluation and Research in the Employment and Training Administration, the evaluation tracks a large sample of 1977 corpsmembers and a carefully selected comparison group for two years after the entry point, or an average of 18 months after the corpsmembers have left the program.

Significant benefits are documented:

Impacts of Job Corps during 12-18 month period after termination

	Job Corps	Control	Net Difference
o Percent time employed (civilian)	.549	.471	+ .078
o Earnings per week (civilian)	$82.17	$72.48	+$9.69
o Probability of being in military	.090	.051	+ .039
o Probability of having high school diploma or GED	.359	.109	+ .250
o Probability in college	.026	.001	+ .025
o Number moves excluding Job Corps	.409	.194	+ .215
o Percentage receiving any financial assistance	.067	.120	- .053
o Probability of arrest	.070	.078	- .008
o Probability of having illegitimate child (total post program period)	.191	.216	- .025

Further confirming the impact of the comprehensive treatment, there are very significant differences in the outcomes for program completers, partial completers and early dropouts.

Job Corps Net Impacts for Males During the 12-18 month Period After Termination

	Program Completers	Partial Completers	Early Dropouts
Percent of time employed	+ .155	+ .044	+ .047
Probability of military service	+ .087	+ .058	+ .032
Probability of having a high school diploma or GED	+ .399	+ .143	+ .049

Some previous studies have suggested a decay in Job Corps impacts over time. The follow-up results indicate the opposite--most differentials actually increase with time except for impacts on criminality.

Net Impacts of Job Corps by Months After Termination

	0-6	6-12	12-18	18-24
Percent employed	+ .012	+ .051	+ .078	+ .090
Earnings	+$2.02	+$6.00	+$9.69	+$9.03
Percent in military	+ .019	NA	+ .039	NA
Percent in college	+ .014	+ 0.22	+ .025	+ .030
Percent receiving financial assistance	- .054	- .055	- .053	- .056
Number civilian arrests in last six months	- .030	- .028	- .008	- .004

These and other benefits--when valued, projected and discounted under the most reasonable assumption--exceed the costs to society of the substantial investment in Job Corps. While the study presents a variety of alternative assumptions and resulting benefit-cost estimates, the "benchmark" assumptions yield a benefit-cost ratio of 1.38. Put another way, the returns to society for each corpsmember have a present value which is $1,927 above the costs of participation. While there are always uncertainties, the size of this increment provides reasonable certainty that the Job Corps investment in human resources is profitable.

Job Corps compares favorably with all other youth programs. The most rigorous comparable assessment to date has been an analysis of the supported work demonstration which provided structured and supervised work experience to a youth population very similar to that in Job Corps. In contrast to the positive post-program impacts for corpsmembers, participants in supported work realized very minimal gains relative to controls. The social benefits did not exceed the costs under the same assumptions as in this evaluation. In other words, work experience does not have the same long-range impacts as intensive education and training.

This study is one of "knowledge development" activities mounted in conjunction with research, evaluation and development activities funded under the Youth Employment and Demonstration Projects Act of 1977. The knowledge development effort will result in literally thousands of written products. Each activity has been structured from the outset so that it is self-standing but also interrelated with a host of other activities. The framework is presented in A Knowledge Development Plan for the Youth Employment and Demonstration Projects Act of 1977, A Knowledge Development Plan for the Youth Initiatives Fiscal 1979 and Completing the Youth Agenda: A Plan for Knowledge Development, Dissemination and Application for Fiscal 1980.

Information is available or will be coming available from these various knowledge development efforts to help resolve an almost limitless array of issues. However, policy and practical application will usually require integration and synthesis from a wide range of products, which, in turn, depend on knowledge and availability of these products. A major shortcoming of past research, evaluation and demonstration activities has been the failure to organize and disseminate the products adequately to assure the full exploitation of the findings. The magnitude and structure of the youth knowledge development effort puts a premium on structured analysis and wide dissemination.

As part of its knowledge development mandate, therefore, the Office of Youth Programs of the Department of Labor will organize, publish and disseminate the written products of all major research, evaluation and demonstration activities supported directly by or mounted in conjunction with OYP knowledge development efforts. Some of the same products may also be published and disseminated through other channels, but they will be included in the structured series of Youth Knowledge Development Reports in order to facilitate access and integration.

The Youth Knowledge Development Reports, of which this is one, are divided into twelve broad categories:

1. Knowledge Development Framework: The products in this category are concerned with the structure of knowledge development activities, the assessment methodologies which are employed, the measurement instruments and their validation, the translation of knowledge into policy, and the strategy for dissemination of findings.

2. Research on Youth Employment and Employability Development: The products in this category represent analyses of existing data, presentation of findings from new data sources, special studies of dimensions of youth labor market problems, and policy issue assessments.

3. Program Evaluations: The products in this category include impact, process and benefit-cost evaluations of youth programs including the Summer Youth Employment Program, Job Corps, the Young Adult Conservation Corps, Youth Employment and Training Programs, Youth Community Conservation and Improvement Projects, and the Targeted Jobs Tax Credit.

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4. Service and Participant Mix: The evaluations and demonstrations summarized in this category concern the matching of different types of youth with different service combinations. This involves experiments with work vs. work plus remediation vs. straight remediation as treatment options. It also includes attempts to mix disadvantaged and more affluent participants, as well as youth with older workers.

5. Education and Training Approaches: The products in this category present the findings of structured experiments to test the impact and effectiveness of various education and vocational training approaches including specific education methodologies for the disadvantaged, alternative education approaches and advanced career training.

6. Pre-Employment and Transition Services: The products in this category present the findings of structured experiments to test the impact and effectiveness of school-to-work transition activities, vocational exploration, job-search assistance and other efforts to better prepare youth for labor market success.

7. Youth Work Experience: The products in this category address the organization of work activities, their output, productive roles for youth, and the impacts of various employment approaches.

8. Implementation Issues: This category includes cross-cutting analyses of the practical lessons concerning "how-to-do-it." Issues such as learning curves, replication processes and programmatic "batting averages" will be addressed under this category, as well as the comparative advantages of alternative delivery agents.

9. Design and Organizational Alternatives: The products in this category represent assessments of demonstrations of alternative program and delivery arrangements such as consolidation, year-round preparation for summer programs, the use of incentives, and multi-year tracking of individuals.

10. Special Needs Groups: The products in this category present findings on the special problems of and the programmatic adaptations needed for significant segments including minorities, young mothers, troubled youth, Indochinese refugees, and the handicapped.

11. Innovative Approaches: The products in this category present the findings of those activities designed to explore new approaches. The subjects covered include the Youth Incentive Entitlement Pilot Projects, private sector initiatives, the national youth service experiment, and energy initiatives in weatherization, low-head hydroelectric dam restoration, windpower, and the like.

12. Institutional Linkages: The products in this category include studies of institutional arrangements and linkages as well as assessments of demonstration activities to encourage such linkages with education, volunteer groups, drug abuse, and other youth serving agencies.

In each of these knowledge development categories, there will be a range of discrete demonstration, research and evaluation activities focused on different policy, program and analytical issues. In turn, each discrete knowledge development project may have a series of written products addressed to different dimensions of the issue. For instance, all experimental demonstration projects have both process and impact evaluations, frequently undertaken by different evaluation agents. Findings will be published as they become available so that there will usually be a series of reports as evidence accumulates. To organize these products, each publication is classified in one of the twelve broad knowledge development categories, described in terms of the more specific issue, activity or cluster of activities to which it is addressed, with an identifier of the product and what it represents relative to other products in the demonstrations. Hence, the multiple products under a knowledge development activity are closely interrelated and the activites in each broad cluster have significant interconnections.

This volume should be read in conjunction with other evaluations of Job Corps in Assessments of Job Crops Performance and Impacts. In addition, Enhanced Work Projects--The Supported Work Approach for Youth includes an evaluation structured quite similar to this which indicates the comparable benefits of a work experience approach.

Robert Taggart
Administrator
Office of Youth Programs

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EXECUTIVE SUMMARY

SECOND FOLLOW-UP REPORT OF THE EVALUATION OF THE ECONOMIC IMPACT OF THE JOB CORPS PROGRAM

This is the second follow-up report of the main findings from a study designed to provide the Department of Labor with comprehensive evaluations of both the short-term economic impact of the Job Corps program on participants and the benefits and costs of the program. The information provided herein is based on the most comprehensive data yet available to conduct a study of Corpsmembers. Comprehensive interviews were first conducted in the spring of 1977 with a sample of Corpsmembers then participating in the program and with a comparable group of disadvantaged youths who had not attempted to enroll in Job Corps. At periods 9 and 24 months after the baseline survey, reinterviews were conducted with all of the youths in the comparison group and with Corpsmembers who had been out of the program for a long enough time to provide useful postprogram information.

The baseline survey obtained detailed information on the demographic characteristics of the youths, their social-economic backgrounds, and their work histories and related activities beginning six months before the Corpsmembers enrolled in the program and continuing up to the date of the interview, which represented approximately six months of program experience. The two follow-up surveys continued to collect detailed information on work histories and related activities during the postprogram period when Corpsmembers had been out of the program from one to two years (an average of slightly over 18 months). Altogether, the data base for this evaluation has both baseline and follow-up data on approximately 5,100 youths.

The most important findings covered in this report can be highlighted as follows:

1. The findings on Corpsmembers' postprogram behavior are generally consistent with the hypothesized economic impacts and the important program goal of improving participants' economic prospects. During the first two postprogram years, we find that Job Corps is at least moderately successful overall in achieving its desired effect of (1) increasing employment and earnings, (2) improving future labor-market opportunities, work experience, education, training, health, geographic mobility, and military service, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality.

2. Some of the most noteworthy effects of Job Corps on the behavior of former participants during their second postprogram year are (on a per Corpsmember basis): (1) an increase in employment of over four weeks per year, (2) an increase in earnings of approximately $500 per year, (3) a 4 percentage-point increase in military service, up from 5 percent to 9 percent, (4) an increase in the probability of having a high school diploma or equivalent degree, from 11 percent to 36 percent, (5) higher college attendance equivalent to an increase of nearly 5 full-time college students for every 100 youths enrolled in Job Corps, (6) a reduction in the receipt of financial welfare assistance, amounting to nearly three weeks per year, and (7) a reduction in the receipt of Unemployment Insurance of over one week per year.

3. The positive, overall impacts generally persist through the second year of postprogram observation. The trend over the two-year postprogram observation period appears to be an increase in program benefits during the first few months (especially for employability during the transition from center life to re-entering the regular labor market), and then relatively stable effects through the rest of the two-year period. The one exception is for criminality, which shows relatively large reductions in the very early post-program period that fade out rapidly after Corpsmembers are out of the program for a year. For employment and earnings we find very stable estimates of gains among civilians, especially program completers, for months 6 to 20 in the post-program period--with no evidence of deterioration in impacts over this period but, in fact, substantial growth in program effects on employment and earnings when the increasing military gains are incorporated.

4. Differential impacts among Corpsmembers are found to be associated with sex, family responsibility, and program-completion categories. Relatively larger impacts for males are found for the receipt of Unemployment Insurance and the probability of being in military service, while relatively larger impacts for females without children are found for civilian employment and earnings, the receipt of welfare, and education. The estimated Job Corps impacts for females with children are generally much smaller than for either males or females without children. A substantial, positive correlation is found between the estimated Job Corps impacts and the proportion of the Job Corps program completed. Program completers consistently benefit the most, particularly in terms of employment, earnings, and dependence on welfare. Partial completers are found to benefit little or not at all. Furthermore, these differential impacts by completion category seem to be at least partially attributable to the effect of staying in the program longer and completing the program, which indicates the potential for additional benefits to the program from increased lengths of stay and completions.

5. We find additional differential impacts associated with program and center-related variables, although the causality of these differences cannot be attributed with any degree of accuracy. Completing a GED program is positively associated with the beneficial impacts, and, most importantly, the magnitude of this observed relationship is approximately the same as that for receiving a regular high school diploma. Differential impacts are also found to be associated with the industrial type of vocational training received and the characteristics of the centers that Corpsmembers attended (e.g., the sexual composition, which suggests that a more equal sexual composition would increase program completions and subsequent employment and earnings).

6. In an exploratory analysis we find significant Job Corps effects in terms of reducing extra-marital children and delaying family formation for females. Furthermore, our estimates of overall Job Corps impacts are probably biased downward by not incorporating either the Job Corps effects on family composition or the employment and related effects for females with children.

7. The findings from a comprehensive evaluation of the social benefits and costs of Job Corps suggest that public investment in Job Corps is economically efficient. Our benchmark estimate is that the value of benefits in fiscal year 1977 exceeded costs by almost $2,000 per Corpsmember, or by approximately 39 percent of costs. Furthermore, the program is found to be economically efficient under a wide range of alternative assumptions and estimates. Because

over 40,000 youths enrolled in Job Corps during fiscal year 1977, our benchmark estimate of the net social benefit for the entire program is approximately $80 million for that year.

8. We estimate that over 60 percent of the social benefits come from increases in the value of output that Corpsmembers produced. Another 30 percent of the social benefits are attributable to reductions in criminal activity among Corpsmembers, particularly burglary and larceny.

9. In assessing the distribution of benefits and costs we find a net transfer from non-Corpsmembers as a group (everyone in society other than Corpsmembers) to Corpsmembers. The main economic benefits to Corpsmembers are derived from increased earnings (approximately 70 percent of the benefits) and transfers received while they are in Job Corps. The main economic benefits to non-Corpsmembers are derived from reductions in Corpsmembers' criminal activities and in their use of transfer programs.

10. Sensitivity tests were undertaken for a wide variety of the assumptions and estimates that are used in the benefit-cost analysis, and these sensitivity tests generally confirm that Job Corps is an economically efficient program. With respect to the critical parameter of future growth or fadeout of effects, we find that Job Corps is an economically efficient use of resources as long as the earnings effects do not decay more rapidly than 37 percent per year after our observation period.

11. We find that center operating expenses per Corpsmember are determined largely by center size, the industrial mix of vocational training offered, and institutional factors associated with center administration (i.e., CCCs versus contract centers). Other factors such as the geographic location and coeducational status of centers appear to be somewhat important but are more difficult to isolate. The largest differences in operating expenses are associated with scale economies of larger center size up to approximately 600 Corpsmember positions at a center.

While the estimates are not exact and single numbers do not generalize very well, after a careful analysis we are relatively confident about the broad implications of our findings for disadvantaged youths in general and for Job Corps in particular. In the report, we present detailed discussions of all of the findings summarized above.

Chapter I introduces the report and briefly indicates the availability of other reports from our evaluation of the Job Corps program. Chapter II provides an overview of the Job Corps program and our evaluation. Chapter III presents overall findings on whether Job Corps is achieving its hypothesized effects--especially with respect to its goal of improving Corpsmembers' economic prospects. Chapter IV summarizes brief exploratory analyses of (1) the effects of Job Corps on family composition and (2) the evidence to date on employment and related impacts for females with children. The comparative evaluation of Job Corps benefits and costs is summarized in Chapter V. Chapters VI and VII discuss differential program impacts for, respectively, benefits and costs. Chapter VIII examines issues with respect to drawing general inferences about Job Corps from our data base. Finally, Chapter IX offers some concluding remarks. In addition, 14 other reports that document specific topics in more detail are available from this evaluation.

I. INTRODUCTION

This is the second follow-up report of a study designed to provide the Employment and Training Administration of the U.S. Department of Labor (DOL) with a comprehensive evaluation of the short-term economic impact of the Job Corps program. The two major issues addressed in this report are (1) the short-term economic impact of Job Corps on program participants, and (2) whether the beneficial impacts of Job Corps as a whole outweigh the costs of the program. In addition, a number of related issues are also addressed, including how long the program effects last, what differential impacts exist among Corpsmembers and centers, and how expenditures and resource use vary among centers.

The information on which this study is based is drawn primarily from three surveys that collected relevant data both from Corpsmembers and from a comparison sample composed of youths who did not participate in the program but who were similar to Corpsmembers along other characteristics. The first survey (the baseline interview) was administered during April-June 1977 to a cross-section of Corpsmembers residing at centers and to the comparison sample. The second survey (the first follow-up interview) was administered approximately 9 months later to the comparison sample and to youths in the Job Corps sample who had been out of the program for at least 5 months. The third survey (the second follow-up interview) was administered approximately 15 months after the first follow-up survey, again to the same comparison sample, but this time to youths in the Job Corps sample who had been out of the program for at least 12 months. Over 4,300 youths were interviewed for the Second Follow-Up Survey, and, altogether, the data base for this evaluation has both baseline and some follow-up data on approximately 5,100 youths.

The questions in the three surveys were designed to obtain detailed longitudinal information on the following topics:

- General demographic characteristics
- Socioeconomic background
- Employment and earnings
- Military service
- Education and training
- Geographic mobility
- Health status
- Receipt of public assistance
- Receipt of Unemployment Insurance and Worker's Compensation
- Antisocial behavior

In addition, information was obtained on Corpsmembers' ratings of the program (see Mallar et al., 1978) and on where respondents could be reached for future interviews.

The next chapter, Chapter II, provides an overview of the Job Corps program and our evaluation. The first part of Chapter II describes the program setting in which the evaluation takes place, including descriptions of the goals of the program, the main Job Corps institutions, the types of individuals who are served by the program, the types of services provided at centers, the size of the program at the time of our study, changes in the program since our study began, and the current direction of changes in Job Corps. In particular, this discussion focuses on the program's goal of increasing the employability of youths who begin the program with severe employment problems, and on what approaches are used in Job Corps to achieve that goal. The second part of Chapter II summarizes our evaluation design in the context of the two main analytical components: the impact on participants and the benefit-cost comparison. This discussion focuses on (1) the policy and research issues underlying the evaluation, (2) our theoretical approach, and (3) the sample design and survey implementation.

Chapter III presents overall findings on whether the Job Corps program is achieving its hypothesized effects--especially with respect to its goal of improving Corpsmembers' economic prospects during the first two years after Corpsmembers leave the program. Specifically, empirical findings are presented on whether Job Corps is successful in (1) increasing employment and earnings, (2) improving future labor-market opportunities; (3) reducing dependence on public assistance and other public transfers, and (4) reducing criminality. Furthermore, this chapter presents the first statistically reliable information on the duration of Job Corps impacts, (that is, of course, under the limitations imposed by an average of only 18 months of postprogram follow-up information).

Chapter IV summarizes (1) a brief, exploratory analysis of the effects of Job Corps on family composition and (2) the evidence to date on the employment and related impacts for females with children. Job Corps effects on family composition have never before been analyzed, and this chapter presents some of the first empirical evidence on the relevance of this issue to youth training programs. In addition, as our sample ages, more of the youths begin to take on family responsibilities--hence, the impact on employment and related activities for females with children takes on greater importance.

The comparative evaluation of Job Corps benefits and costs is summarized in Chapter V. This chapter develops estimates of the value of

Job Corps effects by combining the estimates of postprogram impacts from Chapter III with secondary data on the values, or prices, of these effects. Program costs are then estimated with financial data from the Job Corps financial reporting system, from data we collected at individual centers, and from the U.S. Office of Management and Budget. Finally, estimates of the program's net present value are made under a benchmark set of assumptions about the rate of discount for future benefits and what happens to impacts beyond the observation period, the sensitivity of which is then tested by varying the most speculative of these underlying assumptions.

Chapters VI and VII discuss differential program impacts. In Chapter VI, we examine differences in program effects among subgroups of Corpsmembers and centers. In Chapter VII, we investigate differences in expenditures and resource use among centers. In Chapter VIII we examine issues with respect to drawing general inferences about Job Corps from existing data. This chapter discusses the generalizability of the findings presented in previous chapters. Specifically, we address issues with respect to an interpretation of our findings in the context of a changing program, and the sensitivity of our findings to the econometric procedures, interview nonresponse, etc. Finally, Chapter IX offers some concluding remarks.

Currently available are several other reports that were generated from this evaluation of the economic impact of the Job Corps program, and the interested reader should consult all appropriate volumes.[1/] These additional reports are listed by title after the table of contents, and many are referred to in the text of this report. There are three other primary reports and fourteen supplemental technical reports.

The three additional primary reports include (1) the Interim Report (which covers baseline data and assesses the adequacy of the comparison sample), (2) An Examination of Job Corps Participation (which describes Corpsmembers and examines their ratings of the program), and (3) the First Follow-Up Report (which covers the first postprogram findings based on the First Follow-Up Survey). The fourteen technical reports cover a wide range of topics that can be grouped into three broad areas, as follows: (1) supplemental reports on sampling and survey procedures (four reports: Technical Reports A, B, H, and L); (2) supplemental reports providing details and derivations of evaluation findings and econometric procedures from the main follow-up reports (six reports: Technical Reports C, D, E, F, J, and K); and (3) supplemental reports on secondary topics that are outside the main focus of the evaluation, but which are of important policy interest and can be addressed with the data collected as part of our evaluation of Job Corps (four reports: Technical Reports G, I, M, and N). The four sampling and survey reports include one on sample design and implementation, two on survey methods and results, and one on nonresponse to the interviews. The six technical reports providing additional detail include one on econometric methodology, two on a comparison of benefits and costs, one on the value of output in work projects, one on resource usage at centers, and one on program operating costs. The four secondary reports include two on an experiment with incentive payments to survey respondents, one on comparisons between Job Corps and other youth programs, and one on Job Corps MIS data.

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[1] All these reports are available from the Office of Publications at MPR for the cost of reproduction.

II. OVERVIEW OF JOB CORPS AND THE EVALUATION

Job Corps is a major public program that attempts to alleviate the severe employment problems faced by disadvantaged youths in the United States--especially those who live in poverty areas.[1/] Youth employment problems, while always a serious concern, have recently become more severe because of increases in the teenage population and the persistent downturn in our economy. During the time period covered by our analysis (1977 to 1979), an average of four out of every ten black youths between the ages of 16 and 21 who were in the labor market were unemployed. Moreover, recent surveys have shown that in the poverty areas of central cities, among all black youths fewer than two out of every ten have jobs.

A. THE JOB CORPS PROGRAM IN 1977[2/]

The Job Corps approach is to provide a comprehensive set of services that include "vocational skills training, basic education, health care, and residential support for young people who are poor, out of school and out of work. Its aim is to break the cycle of poverty permanently by improving

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[1/] The term disadvantaged is used throughout this report to refer to the set of youths who have employability problems caused by their socioeconomic background. Thus, it embodies several factors related to age, educational level, income status, race-ethnicity, employment history, previous social behavior, etc., that limit the ability of young men and women to obtain and hold jobs.

[2/] This chapter draws very heavily from three documents prepared by the national Job Corps staff: (1) "Job Corps in Brief, FY-77," 1978; (2) "A Planning Charter for the Job Corps," 1978; and (3) "The Expansion and Enrichment of the Job Corps," 1978. The interested reader should refer to these papers, as well as to Kerachsky and Mallar (1978) and Mallar et al. (1978), for further details. Also, Levitan and Johnston (1975) have summarized the first ten years (1964-74) of Job Corps operations.

lifetime earnings prospects."[1] Job Corps is designed to serve youths who currently live in such debilitating environments that they must be relocated to residential centers to benefit from basic education, vocational training, and ancillary services.[2] Education and training conducted in a supportive environment are the key elements in the program's effort to improve the employability of disadvantaged youths, which, in turn, will help them become more productive and responsible citizens.

1. Institutional Setting

The Job Corps program was originally established by the Economic Opportunity Act of 1964. Control of the program was later transferred (in 1969) from the Office of Economic Opportunity to the Department of Labor (DOL), and Job Corps was eventually incorporated without changes as Title IV in the Comprehensive Employment and Training Act (CETA) enacted in 1973. While there has been a general decentralization and decategorization of the other employment and training programs under CETA, Job Corps is still administered primarily at the federal level. Its incorporation into CETA, however, has resulted in the transfer of direct responsibility for program operations and center contracting to DOL's regional employment and training offices.

There are two basic types of Job Corps centers: those operated by private contractors selected in a competitive bidding process which is conducted by the regional offices, and those located on public lands

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[1] Quoted from "The Expansion and Enrichment of the Job Corps," U.S. Department of Labor, Employment and Training Administration, 1978, p. 1.

[2] Some of the Job Corps centers in urban locations have added a few nonresidential slots (i.e., positions) in the 1970s. However, the nonresidential components of Job Corps were not included in our evaluation and, hence, will not be considered in this report. The nonresidential components were excluded because the limited funds available for this project would be more productively allocated to the residential slots, and residential Corpsmembers include approximately 95 percent of all Corpsmembers.

(predominantly in national parks and forests) and operated by the Department of Agriculture or the Department of the Interior. The former centers are usually referred to as "contract centers," and the latter as "civilian conservation centers" (CCCs). In fiscal year 1977, there were sixty-one centers in operation, located in thirty-two states and Puerto Rico: twenty-seven CCCs; two CCC-type centers operated by the Commonwealth of Puerto Rico; thirty centers operated under contracts with private business firms, nonprofit organizations, and state and local government agencies; and two extension centers for advanced vocational training operated by unions.[1/] Two contract centers had just opened during the year (a new center in Mississippi and a relocated center in New York).[2/]

Recruitment and placement activities are carried out under contracts with employment service offices, various unions, local schools, volunteer agencies such as Women in Community Service, Inc. (WICS) and Joint Action in Community Service, Inc. (JACS), and special private agencies,[3/] in addition to the efforts of individual centers and the

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[1/] One extension center is operated by the Brotherhood of Railway Airline and Steamship Clerks (BRAC) of the AFL/CIO; the other is operated by Stewards Training and Recreation, Inc., of the Marine Cooks and Stewards Union of the AFL/CIO. In addition, several unions (particularly in construction trades) have contracts to provide training at the other centers (at all CCCs and some of the contract centers).

[2/] Other centers have since opened and more centers are scheduled to open in the near future to enable Job Corps to achieve its expansion goal of doubling the number of slots compared to fiscal year 1976 (see Section E below). The focus of the MPR evaluation of Job Corps and this report is on all centers operating in the continental United States during fiscal year 1977. Chapter III and Technical Reports A and B present more detailed discussions of the sample used in this evaluation.

[3/] Private contracting agencies such as the separate GATE-house (Graduate Aid to Employment for Ex-Corpsmembers) contractors were operating in six large metropolitan areas (Atlanta, Baltimore, Los Angeles, New York, Philadelphia, and Washington, D.C.), where many ex-Corpsmembers reside after they leave the centers.

regional offices. These groups (especially the volunteer agencies and special private agencies) often provide other support services to youths who have recently left Job Corps, to facilitate their transition from center living to a job and regular living arrangements.

2. Enrollees in Job Corps[1]

Data gathered as part of this evaluation show that the youths served by Job Corps are severely disadvantaged. Prior to enrolling in the program, the Corpsmembers have relatively low levels of educational attainment and employment. In addition, they have relatively high incidences of welfare dependence and brushes with the law. The combined effect of these characteristics limits the ability of these young people to obtain and hold productive jobs.

A review of the socioeconomic characteristics of youths in Job Corps during the spring of 1977 shows that:

- Approximately one-half of the Corpsmembers were under age 18 at the time they enrolled, and nearly one-quarter were 16 (the statutory age limits were and continue to be 14 to 21, but very few youths under age 16 were then or are now admitted).
- Approximately 70 percent of the Corpsmembers were male (however, efforts are currently being made to increase female participation to 50 percent of the total enrollment).
- Over 75 percent came from minority backgrounds--59 percent black, 11 percent Hispanic, 5 percent American Indian, and less than 1 percent Asian or Pacific Islander.
- Between 85 and 90 percent of the Corpsmembers had not completed high school at the time they enrolled.

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[1] For a more detailed description of Job Corps participants, see Kerachsky and Mallar (1978).

- Almost all Corpsmembers had experienced difficulties in obtaining and holding jobs; moreover, when they did find work, the jobs usually did not pay well. Over one-third of the enrollees never had a job at which they worked at least twenty hours per week and which lasted for at least one month. In the six months before enrolling in Job Corps, the typical Corpsmember was employed less than one-third of the time and averaged fewer than 12.5 hours of work per week at a wage rate ($2.81) that was only slightly above the federal minimum.

- Almost all Corpsmembers had experienced poverty, welfare dependence, or both; in the six months before enrolling in Job Corps, over 90 percent either had incomes that were below the poverty line or were receiving welfare assistance.

- While many (28 percent) had attempted to enlist in the military service, most of them failed to qualify (85 percent of those who attempted).

- Many Corpsmembers had a brush with the law--at least 38 percent had been arrested at some time before enrolling, and 19 percent had been convicted (about one-half of those Corpsmembers who had been arrested).

3. Types of Services Provided at Centers

To help Corpsmembers overcome the problems highlighted above, Job Corps attempts to provide a comprehensive program that is flexible enough to meet the individual needs and problems of each disadvantaged youth. The components of the Job Corps program include remedial education, high school equivalency classes, vocational training, health care and education, residential living, and counseling and other ancillary services, each of which is incorporated into a unified framework tailored to meet the individual needs of each youth.

Education. The Job Corps education program has evolved with the intent of meeting the varied deficiencies in the backgrounds of Corpsmembers and to enable them to proceed at the maximum pace commensurate with their abilities. The education program includes remedial education (emphasizing reading and mathematics), World of Work (including consumer education, driver education, home and family living, health education, and programs designed for individuals whose primary language is not English), and General Educational Development (GED) for Corpsmembers who are academically qualified. The GED certificate is recognized by state educational agencies as the equivalent of a high school diploma. The Job Corps encourages and emphasizes the GED program "for those who are academically qualified. In fiscal year 1977, over 4,000 enrollees were awarded the General Education Development Certificate."[1]

Vocational Skills Training. Like the education program, the training program at Job Corps centers is designed to meet individual needs and problems and to enable Corpsmembers to advance at the maximum pace commensurate with their abilities. Therefore, all the training programs provide for an open entrance and exit capability and are continually being reviewed and revised in order to keep up with the changing needs of Corpsmembers, as well as with the changing labor market.

There are some notable differences between vocational training programs at CCCs and those at contract centers. The training programs at CCCs are often operated by unions and tend to be of a "hands-on" work-project nature, with actual construction and production taking place.[2] In contrast, the training programs at contract centers are more often operated by the centers themselves or by individual private subcontractors, and the training tends to be of a classroom-instruction, shop-type, or "mock-up" nature, with some work-experience positions available upon the successful completion of the training.

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[1] Job Corps in Brief, FY-77, p. 3.

[2] Most of the union instructors use curricula approved for the first two years of the union's apprenticeship program.

Health Care and Education. Comprehensive health services are provided to all enrollees, including medical examinations (with follow-up treatments, if necessary), immunization, dental examinations (for all Corpsmembers who stay at least ninety days) and dental treatment, professional help for emotional and other mental-health problems, and instruction in basic hygiene, preventive medicine, and self-care. Health education is also given high priority in Job Corps, with the aim of preparing Corpsmembers "to make responsible decisions regarding health and health-related matters by providing them with relevant, factual information."[1/]

Residential Living. Residential living is a key component of the Job Corps program and distinguishes it from most other public employment and training programs. The concept behind residential living is that the target population comes from such debilitating environments that they need a new and more supportive environment to derive the intended benefits of the vocational training and education courses. The residential-living program (including meals, health services, dormitory life, entertainment, sports and recreation, center government, center maintenance, and other related activities) is "planned to help new Corpsmembers adapt to center life, motivate and support constructive attitudes and lifestyles, and prepare them to function effectively in the outside world. . . . It involves such complex areas as relationships among racial and ethnic groups, motivation of alienated or discouraged young people, adaptation to unfamiliar group living situations, adult-youth cooperation in an

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[1/] Job Corps in Brief, FY-77, p. 3.

institutional setting, and the role of peer groups in influencing conduct and attitudes."[1]

Counseling and Other Ancillary Services. The centers provide counseling services and residential advisors both to help Corpsmembers plan their educational and vocational curricula and to motivate Corpsmembers and create a supportive environment. Some of the other support services provided by Job Corps (for example, during recruitment, placement, and the transition to regular life and jobs) were discussed above.

4. Size of Job Corps

At the start of fiscal year 1970 the Job Corps program was cut back drastically in terms of both financial expenditures and the number of youths served. From then until fiscal year 1977 the budget was held roughly constant in nominal amounts, and the number of youths served stabilized at approximately 21,000 to 22,000 slots (i.e., positions) and 45,000 new enrollees annually. However, over the same time period, inflation greatly eroded the real purchasing power of that budget (held fixed in nominal amounts); consequently, capital equipment was allowed to deteriorate in order to serve the same number of youths within the more restrictive purchasing power.

With the decision in fiscal year 1977 to renovate and expand Job Corps (see the next section), the budget and number of slots in the program were increased. In fiscal year 1977 the budget was increased 58 percent in nominal terms, to $274 million, while the applied funding (i.e., the costs actually incurred rather than budgeted) increased by 23 percent, to $231 million. Some of the additional expenditures began to be allocated to the planning of expansion, actual expansion, improvements in services, staffing increases, and the repair and replacement of capital equipment that had been allowed to deteriorate during the previous seven years. Similarly, the number of slots in the program rose 7 percent in fiscal year 1977, to 22,225 slots, with the addition of one new center and a small amount of expansion at some existing centers. However, the number of new youths enrolled declined slightly in fiscal year 1977 because the turnover rate fell; hence, the average length of stay in Job Corps and the proportion of program completers increased during that year (conceivably as a result of improvements made possible by the additional resources).

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[1] Job Corps in Brief, FY-77, pp. 4 and 5.

5. Current Trends

The most recent trends in Job Corps are dominated by plans to expand the program. Job Corps began increasing its capacity in fiscal year 1977 in response to a congressional authorization to double the size of the program as recommended by DOL--from its fiscal year 1977 level of 22,000 slots to 44,000 slots by the end of fiscal year 1978. The national Job Corps staff expects to reach the full capacity enrollment of 44,000 by the middle of fiscal year 1981, and the expansion is 75 percent complete now.

In deciding how best to provide the additional program slots, the program has taken several factors into account. First, positions are being allocated across the country according to the relative needs of the various regions (need is determined from recent data on the incidence of poverty and unemployment among youths). Second, the new slots will be allocated among the two current types of centers, as well as among other new types. The contract centers will receive the bulk of the new slots; the CCCs will receive only about 5 percent of the new growth (thus, the proportion of CCCs is declining).

Another 5 percent of the new slots will be devoted to industry work-experience programs, and approximately 15 percent of the new slots will be in the Advanced Career Training program, which allows qualified Corpsmembers to attend junior college or technical school. (For the most part, Corpsmembers in these programs will be assigned, at least for administrative purposes, to a regular contract center or CCC.)

In addition to the general expansion of Job Corps, DOL has recently emphasized nine "improvement" areas for Job Corps, as follows (quoted from the Employment and Training Report of the President, 1979, page 170):

- Arrangements have been made with prime sponsors and with the Armed Forces for referrals to Job Corps. Increased outreach is needed to recruit more young women. Screening procedures should be simplified wherever possible while they should also assure that youth who can most benefit from Job Corps are identified.

- Only a minority of Corpsmembers complete training and are placed directly into training-related jobs. Better linkages are needed with the labor market. The Industry Work Experience Program and a variety of newly developed advanced career training programs should assist in this effort.

- New reading and GED programs have been developed and are being introduced, and an experimental college program has been implemented. An Educational Improvement Effort will experiment with alternative education approaches.

- The world-of-work program to provide Corpsmembers with jobseeking and jobholding skills needs to be strengthened. Alternative systems are now being tested.

- A formula for living and readjustment allowances has been derived in response to a congressionally authorized increase while attempting to balance the effects on recruiting, retention, performance reward, readjustment, and equity. This formula is now under review by Job Corps and the Department.
- Comprehensive health services are provided at Job Corps centers, and for 1 in 7 enrollees a previously undetected health condition is identified.
- Food in Job Corps centers is nutritionally sound but apparently not as appealing to most Corpsmembers as could be desired. The elimination of the statutory ceiling on center operating costs will permit modest increases in the amounts spent on food.
- Needed and long-delayed improvements have been made in center facilities to enhance the quality of life in Job Corps.
- The placement system must be reexamined in the coming year with the aim of more closely linking jobs and training as well as shortening the readjustment period.

B. THE EVALUATION DESIGN

The Job Corps program has survived over a dozen years of changing attitudes toward social problems and has emerged as an important component of the current effort to train and employ disadvantaged youths. However, given the relatively large investment per enrollee, surprisingly little was known about the magnitude of most of its economic impacts.[1] Of important concern were the following issues: Does the program provide economic benefits to participants and society? What are the magnitudes of the benefits? Do some Corpsmembers benefit more than others? Do some variants of the program work better than others? Does the total dollar value of benefits outweigh the costs?

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[1] Aside from program data, only the survey conducted by Louis Harris and Associates between 1966 and 1969 has provided economic data on a reasonable-size sample of former Corpsmembers (i.e., with reasonable statistical precision). For a comparison group, however, both program data and the Harris (1969) survey are limited to either early dropouts or "no shows" (i.e., youths who signed up for Job Corps and were admitted, but who never attended). Furthermore, the Harris data are obviously outdated given the subsequent changes in both Job Corps and youth labor markets. (For further details, see Louis Harris and Associates, 1969).

In order to design an evaluation to answer the above questions, we constructed a detailed list of policy and research issues from the hypothesized effects of Job Corps. (These issues are described in the next section of this chapter.) We then used the policy and research issues as a guide in developing an evaluation design (see further below).

1. Policy and Research Issues

The objective of our evaluation is to provide DOL with a comprehensive assessment of the economic impacts of the Job Corps program, especially in the short term. To meet this objective, we must focus on concrete policy and research issues. The issues addressed are as follows:

- The extent to which the Job Corps program provides early economic benefits to its participants in terms of gains in employment, earnings, and other related measures of economic well-being.

- The extent to which participation in Job Corps influences subsequent decisions to enter school, training or work-experience programs, or the military service.

- The extent to which the Job Corps program affects participants' receipt of transfer payments.

- The extent to which participation in Job Corps reduces various forms of antisocial behavior, particularly criminal activities and drug abuse.

- The existence of differential program impacts by type of participant (age, race, sex, prior educational level), by duration of participation in the program, by type of center (size, location, operator), and by components of the program (education received, vocational training, etc.).

- The extent to which program benefits (both during and after program participation) outweigh program costs.
- The satisfaction of Job Corps participants with their program experience, and their assessment of the strengths and weaknesses of the program (see Mallar et al., 1978).

The first five items on this research agenda show the range of potential benefits to participants based on a human capital approach. The magnitudes of economic impact will be measured by comparing the postprogram behavior and economic status of Corpsmembers with what they would have been had the youths not participated in Job Corps. Item 6 requires valuing program benefits and comparing them to the costs. This benefit-cost comparison (or set of comparisons, as we describe in Chapter V) is achieved by aggregating estimates of the dollar values of postprogram benefits with similar measures of in-program benefits, and comparing the total dollar value of program benefits to the total dollar value of program costs. Thus, the benefit-cost research builds upon the impact analysis by assigning dollar values to the estimated program benefits.[1/] Item 7 on the research agenda, which focuses on Corpsmembers' perceptions of the program impacts and their assessments of program-related experiences, was completed as part of the First Follow-Up Report (see Mallar et al., 1978).

2. Analysis of Participant Impacts

The theory of economic choice underlies many studies of employment and training programs. This theory suggests that individuals choose

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[1/] A benefit-cost analysis has the advantage of providing a summary measure that can be used to judge the worth of the program. In addition to providing inputs into the benefit-cost calculations, however, the impact analysis shows program effects that cannot readily be valued in dollar amounts, and allows readers to make their own judgments about the value of various program benefits.

among competing demands on their time according to the wage rates they can receive, other prices, and sources of nonemployment income that are available. A person's wage rate is hypothesized to depend on his or her productivity, which increases with education and vocational training. By providing education and vocational training, Job Corps should increase participants' productivity, wage rates, employment opportunities, and economic incentives to work.[1] However, institutional labor-market factors such as the minimum wage may cause an excess supply of labor in the markets for disadvantaged youths, so that another effect of Job Corps might be to increase the employment of Corpsmembers (because they have increased productivity) without affecting their short-term wage rates.[2]

The effects of Job Corps on several important postprogram activities are studied in Chapter III. These activities can be categorized into four broad areas. The first includes labor-market activities, such as labor-force status, employment, hours worked, wage rates, and earnings. Improvement in this area is considered the primary objective of Job Corps. The second area includes additional training and education. Improvement in this area is an important short-term objective because it is expected to increase employment and earnings in the long-run. The third area is dependence on welfare and other public transfers, and the final area is antisocial behavior. The anticipated changes in these last two areas are related to the changes in employment and earnings (and in training and educational activities). As better opportunities arise in the labor market (and scholastically), we expect a decline in welfare dependence and antisocial behavior. The hypothesized effects of Job Corps in each of the four areas are discussed briefly below and are summarized in Table II.1.

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[1] The effect of an increase in wage rates on economic incentives to work is not completely unambiguous, because higher wages may afford some individuals the opportunity to spend more time in activities other than work. However, most studies of youth labor supply have found work effort to be positively associated with wage rates.

[2] Under this example, the minimum wage bolsters the average wage rate received by disadvantaged youths who are employed. Job Corps training helps former Corpsmembers get to the front of the queue for employment when there is an excess supply of labor, in which case they displace other disadvantaged youths in the short run.

Employment and Earnings. The primary hypothesis is that, other things being equal, young adults who obtain Job Corps training will become more productive and, hence, will receive more employment and higher earnings than those who do not.[1/] The increased productivity is expected to lead to improved employability (as measured by increases in labor-force participation, employment, hours worked per week, and the proportion of weeks worked), as well as to higher wage rates and higher earnings. This hypothesis is based on previous research on the effects of training and education on labor-market activities.

In addition to the short-term impacts after leaving Job Corps, there may be subsequent reinforcing effects. For example, regular employment often provides on-the-job training and a record of worker reliability that

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[1/] Each of the hypotheses developed in this section is based on the difference between the postprogram behavior of Corpsmembers and what their behavior would have been in the absence of any Job Corps training. For ease of presentation, the discussion is sometimes conducted as if there were no underlying differences between the Corpsmember and comparison groups, so that the impacts of Job Corps can be characterized by direct contrasts between the behavior of Corpsmembers and that of comparison-group members. Of course, the statistical techniques used (see Chapter III) attempt to compensate for any underlying differences between the Corpsmember and comparison groups. In addition, all the hypotheses discussed herein are weakened when allowances are made for the alternative training and education programs available to youths. In most of the empirical sections of this report we measure Job Corps impacts relative to what Corpsmember activities would have been had they not participated in Job Corps. In the absence of Job Corps, many Corpsmembers would not have obtained zero treatment but, instead, would have received some amount of alternative training and education that they forego in favor of participating in Job Corps.

TABLE II.1

SUMMARY OF HYPOTHESES FOR PROGRAM IMPACTS ON PARTICIPANTS

Relative to if they had not gone into the program, participants will:

A. Employment and Earnings

1. Have more employment
2. Have more stable employment
3. Have higher earnings
4. Have higher wage rates

B. Investments in Human Capital

1. Be more likely to have productive work experiences
2. Be more likely to return to school or continue their education in other ways, especially at higher levels of education
3. Be more likely to participate in training programs
4. Be healthier
5. Be more geographically mobile
6. Be more likely to qualify for military service

C. Dependence on Welfare and Other Public Transfers

1. Have reduced receipt of cash transfer payments
2. Have reduced receipt of in-kind transfer payments

D. Antisocial Behavior

1. Be less likely to engage in criminal activities
2. Be less likely to abuse drugs or alcohol

is, in turn, rewarded with even higher wage rates and earnings in the future. In contrast, the impacts of Job Corps could fade out over time as the influence of the program becomes less significant the farther removed former Corpsmembers are from the program in time.

Investments in Human Capital. Economists define "investments in human capital" as current activities that lead to future increases in productivity and, hence, earnings potential (indirect program effects on productivity and earnings). In this evaluation we will consider six types of investments in human capital: (1) work experience (see above), (2) education, (3) training, (4) better health, (5) geographic mobility, and (6) military service.

Work-experience, educational, and training programs are important placement alternatives to regular employment for Job Corps terminees, especially for younger Corpsmembers. Many of the younger terminees could still profit from additional work experience, schooling, and training after they leave Job Corps, and, moreover, job placements are often difficult for them. Therefore, both the impact and benefit-cost analyses must take into account any postprogram increases in such investments in human capital. While increased employment and higher earnings continue to be the long-run goals of both the program and participant, work-experience, educational, and training programs are important short-term, intervening factors that may lead to higher employment and earnings in the future.

We hypothesize that former Corpsmembers have higher probabilities of participating in work-experience, educational, and training programs than comparison-group members. However, to the extent that Job Corps succeeds in improving immediate postprogram labor-market opportunities (thereby increasing the opportunity cost of time spent in human capital programs), this hypothesis is weakened. In any case, it is expected that former Corpsmembers will participate in higher-level programs than youths in the comparison group and will be more likely to complete any given level (i.e., more likely to obtain advanced degrees or certificates).

An additional hypothesis that falls into the category of human capital investments is that participation in Job Corps increases geographic mobility. This is supported by the fact that the Job Corps program provides services that help terminees relocate to areas where employment opportunities exist. We also expect that the additional income from earnings, as well as the health education and treatments provided by Job Corps, will lead to the improved health status of former Corpsmembers relative to youths in the comparison group.

The expected effect of Job Corps on enrollment in the military is somewhat ambiguous. It is not clear whether former Corpsmembers should be more or less likely to enlist in the military. They may be more likely to enlist for the investments in human capital associated with military service (e.g., for the vocational-training aspects and broadened experiences), or they may be less likely to enlist because of the increased opportunity cost of their time (i.e., better job opportunities in the civilian labor force). However, we hypothesize that Job Corps terminees who take the Armed Forces Qualifying Test are more likely than comparison-group members to pass the test. In addition, military service is an explicit placement target for some Corpsmembers, and GED training in Job Corps should increase the opportunity and rewards for enlisting.[1]

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[1] The new working relationship between Job Corps and the military, which was initiated by the signing of a memorandum of understanding by the Department of Labor and the Department of Defense on January 13, 1978, should facilitate the enlistment of Job Corps terminees into the military service.

Dependence on Welfare and Other Public Transfers. A set of hypotheses that are closely related to labor-market activities concerns the effects of Job Corps participation on welfare dependence. Of course, Corpsmembers have reduced receipts from welfare while they are at the centers. In addition, because of increased earnings after leaving Job Corps, former Corpsmembers are expected to receive fewer transfers--including AFDC, General Assistance, Food Stamps, public housing, Unemployment Insurance, and Workers' Compensation--than they otherwise would have received during the postprogram period.

These transfer-payment effects of Job Corps may be attenuated (or possibly reversed) if participants become more knowledgeable about the nuances of transfer programs and, consequently, increase their participation in them. In addition, those Corpsmembers who seek additional training or education in the postprogram period may obtain a temporary increase in their transfer payments. Nevertheless, on balance, the amount of transfer payments received by Corpsmembers is expected to be lower than that received by the comparison group both during the program and in the postprogram period.

Antisocial Behavior. Corpsmembers are expected to reduce drug and alcohol abuse and have lower probabilities of engaging in criminal behavior. While the Corpsmembers are at the centers, both of these responses should be very large because their activities are restricted, their behavior is closely monitored, and their material needs are provided; consequently, they have few opportunities and small incentive to engage in drug abuse or crimes. After Corpsmembers leave the program, these reductions in antisocial behavior are expected to continue, but probably at a smaller rate. The postprogram reductions in antisocial behavior stem from the entire Job Corps effort to increase employability in order to promote more regular life-styles--from vocational training and educational services to general counseling and center living. Training and education are important because, to the extent that Job Corps is successful in increasing the employability and the educational abilities of Corpsmembers, legitimate activities become increasingly more attractive relative to illegitimate activities.

3. Comparative Evaluation of Benefits and Costs

The purpose of the comparative evaluation of benefits and costs is to determine whether program benefits outweigh costs. Does society have more goods and services at its disposal because of the investment in Job Corps? The benefit-cost analysis, which is presented in Chapter V, builds upon the results for participant benefits and compares the dollar values of benefits and costs. Implementing a benefit-cost assessment is especially difficult for programs such as Job Corps, which has a wide range of potential effects that could occur over several years. The key elements of our benefit-cost analysis are summarized in Chapter V and presented in more detail in Technical Report K.

4. Evaluation Design

The previous sections summarized the objectives in an evaluation of the economic impact of Job Corps. It should be clear from the discussion that in order to address all of the relevant policy and research issues the study design must be comprehensive. This section summarizes the evaluation design we developed to meet the objectives of the study.

Comparison-Group Methodology. During the design phase of this study, much effort was devoted to selecting an appropriate comparison group. Operational considerations prohibited the random assignment of potential Job Corps enrollees to nonparticipant status. Therefore, considerable effort had to be devoted to developing a suitable group of nonparticipants with which to compare Corpsmember behavior, so that the hypothesized impacts of the program could be tested and the magnitude of the effects of the program estimated.

Within the constraint against randomization and the budget limitations for the evaluation, we had to develop a sample design that would both minimize bias and maximize efficiency in estimating the effects of Job Corps. We had to take into account two important factors: (1) that Job Corps was geographically clustered (in terms of the home areas from which Corpsmembers came and in terms of where the centers were located), and (2) that the Corpsmembers would already be enrolled. The most efficient procedure called for sequential matching--first obtaining appropriate sites and then finding appropriate youths within sites. Finally, we included in the baseline questionnaire detailed information concerning the Corpsmembers' socioeconomic backgrounds, so that the comparability of the Corpsmember and comparison groups could be tested and any differences controlled for in the statistical techniques.

The first step was to eliminate program sites in order to reduce the probability of self-selection biases (e.g., more highly motivated youths enrolling in Job Corps). These were defined as geographical areas that are saturated by Job Corps participation (i.e., high proportions of eligible youths entering the program from a location proximate to a center). The nonsaturated areas were then assigned selection probabilities in proportion to their similarities to the home areas of Corpsmembers, based primarily on the poverty and racial composition of the areas as determined from Census data.1/

1/Socioeconomic characteristics of the home areas of recent Job Corps participants were used to select the locations of the comparison-group sites. The Primary Sampling Units (PSUs) were five-digit zip-code areas

Once the control sites were chosen, youths living in these areas were assigned selection probabilities in proportion to their similarity to Job Corps participants (actual participants and not just Job Corps eligibles), based on their poverty, age, race, and educational status. Names of youths were obtained from school dropout lists and from local employment service offices. Together, these two sources provided an adequate sampling list from the universe of youths who participate in Job Corps. School dropout lists identified young recent dropouts who were similar to approximately 70 percent of the Corpsmembers, and the active files at local employment services provided older youths who had been out of school for a longer time and were similar to the other 30 percent of Corpsmembers. A sample of youths was then chosen to be included in the comparison group, with females oversampled relative to their proportion in Job Corps to increase the efficiency of estimates computed separately by sex.[1]

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in urban locations (Standard Metropolitan Statistical Areas) and three-digit zip-code areas in rural locations. Data from the 1970 Census on population density, geographic location, percent of poverty families, mean family income, housing quality, percent of young (16 to 21) adults, percent of Hispanic youths, percent of black youths, and youth unemployment rates in the PSUs were used to assign selection probabilities. Regression analysis was used to determine which of these variables would best predict the home regions of Corpsmembers. For both three-digit and five-digit zip-codes, the best predictor was the percent of families in the region that had incomes below the poverty level and that were headed by someone younger than 54 years of age. The second best predictor was the percent of minority youths in the region. The percent of poverty families by itself explained nearly 30 percent of the variances in the proportion of Job Corps enrollments by zip-code regions. Probabilities of selection were then assigned to all of the nonsaturated zip-code areas in the United States, proportional to their similarity to the home areas of Job Corps participants, as measured by the percent of poverty families. Proportional stratifications by race and region of the country were also maintained (see Technical Reports A and B for more details).

[1] The target for the male/female ratio was 50/50 in the comparison group, as opposed to the 70/30 split for the Job Corps sample in order to obtain increased precision in separate estimates for females.

This quasi-experimental design seems appropriate for our evaluation and should lead to reasonably precise estimates of the economic impact of the program. The assumptions needed for unbiased and relatively efficient estimates of the program treatment effects seem plausible provided that appropriate statistical techniques are used (see Chapter III).[1] There is no overlap between the Job Corps and comparison-group samples, and the Corpsmember sample should differ from the comparison group primarily in terms of access both to information about Job Corps and to Job Corps centers.[2]

Sample Size and Selection. The sample selection procedures were based on the necessity to balance the evaluation, operational, and cost considerations.[3] For the Job Corps sample, the strategy we chose was to select a random sample of participants in the program at a point in time. For analytical purposes, an enrollment-based sample would have had more appeal, but would have been much more expensive, would have yielded many early dropouts, and would have greatly delayed the research findings.

The sample size was targeted to be large enough to ensure a high probability of observing statistically significant impacts if the "true" Job Corps effects are large enough to be policy-relevant. Specifically, the sample size was chosen to be large enough to have a 90 percent chance of obtaining statistically significant Job Corps effects for employment and related activities if the "true" net present value of Job Corps training is positive (i.e., if the "true" benefit-cost ratio is greater than one). If the Job Corps program is economically efficient, we should (and do--see Chapter III) observe many statistically significant effects for Job Corps on employment and related activities. The sample size for Corpsmembers was targeted to be larger than for the comparison group because of interest in estimating differential program impacts among subgroups of Corpsmembers.[1/]

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[1] "Unbiased," as used here, means that, on average, the estimator should yield a value close to the "true" one. In other words, any biases are both likely to be small and unlikely to affect the substantive findings of our evaluation. Of course, all estimates are biased to some extent because all statistical models are only approximations to reality. "Efficiency" is defined to mean that the estimator has a smaller variance than any other with the same (or smaller) amount of bias and using the same data.

[2] The comparison-group methodology is further explained and assessed in Technical Reports A and C. See also Kerachsky and Mallar (1978).

[3] The sample design is chosen to minimize the cost of obtaining the desired level of statistical precision for estimates of Job Corps effects (see Technical Report A).

To obtain an area probability sample, we used standard procedures to randomly select approximately one-third of the Corpsmembers in the program during April 1977. Each Corpsmember then at a center had an equal probability of being selected (approximately one-third).[2/] For the baseline survey, 5,297 Corpsmembers were selected, and 5,133 of those were interviewed (completed interviews) during April and May of 1977. The first follow-up sample included everyone from the original sample who had left Job Corps before October 15, 1978 and, hence, who had been out of Job Corps for at least five months (2,887 youths), and 2,417 Corpsmember interviews were completed. The second follow-up sample included all those who had left Job Corps before April 15, 1978

[1/] For further details and justification, see Technical Report A.

[2/] There were only two exclusions from the sampling frame--youths or centers outside the continental United States, and nonresident Corpsmembers. Justifications for these sample exclusions are presented below.

(4,349 youths), and 3,042 Corpsmember interviews were completed.[1] The survey response rates for the Job Corps samples were 97 percent of Corpsmembers sampled at baseline, 84 percent of baseline completions at the first follow-up, and 79 percent of first follow-up completions at the second follow-up (70 percent of the baseline sample, cumulative).[2] For the comparison sample, 1,496 youths were interviewed at baseline, 1,306 at first follow-up, and 1,267 at second follow-up.

Note that Corpsmembers who drop out of the program early are less likely than program completers to be at a center at any point in time; hence, they will be underrepresented by point-in-time sampling such as ours.[3] With our point-in-time sampling, there are proportionally more program completers in the sample of Corpsmembers than would generally be obtained from a sampling frame based on all enrollees. For all enrollees in fiscal year 1977, approximately 40 percent were classified as early dropouts (terminated during their first ninety days), 30 percent were classified as having completed only a portion of the program, and 30 percent were classified as having completed a full program; the corresponding percentages for our second follow-up sample are 9, 35, and 56, respectively. To obtain estimates that are applicable to an average enrollee will necessitate reweighting the data (see Chapter III).

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[1] By the Second Follow-Up Survey, 85 percent of the baseline Job Corps sample had been included in the follow-up sample. The 15 percent who had not been included were composed of partial and full completers with long stays in the program. Their exclusion is cost-efficient for three reasons: (1) they have short postprogram experience, (2) our sample sizes are adequate for these groups without them, and (3) leaving them out approximately offsets any biases from a point-in-time survey over-representing long-term stayers within the completion categories.

[2] Over time, the base for these completion rates include more and more cases that cannot be interviewed (e.g., deceased youths), and the real base shrinks over time. The completion rates are lower for the Job Corps sample at the second follow-up, because one-half the sample was contacted solely by telephone (for further details, see below as well as Technical Reports B and H).

[3] For our purpose, the fundamental difference between "enrollees" and "participants" is that Corpsmembers who stay in the program a long time (i.e., program completers) will be over-represented in participant samples compared to all enrollees. Among Job Corps enrollees, a high proportion (approximately 40 percent) leave the program within ninety days. These early dropouts are replaced continuously by new Corpsmembers, so that a sample of participants at a point in time has a higher proportion of completers than found among enrollees. For the MPR evaluation of Job Corps, a high proportion of program completers is desirable because the impact of the program on early dropouts is probably negligible and differential impacts for different programs and Corpsmembers could occur among program completers. In Chapter III we explain how the observations are reweighted to obtain unbiased estimates for enrollees.

Two exclusions were made from the Job Corps sampling frame--Corpsmembers in centers or from regions outside of the continental United States, and those not residing at centers. This was done for two reasons: (1) those two groups represent only a small proportion of Corpsmembers (less than 1 percent and approximately 5 percent, respectively); and (2) their backgrounds and program treatment seem systematically different from the main group, which would probably require separate analyses (which would necessarily be imprecise) and would reduce the precision of estimates for the main group.

Data Collection. All three research topics require in-depth data on each sample member that must be obtained from interviews. Alternative interviewing strategies were examined to identify the method that would best minimize response errors, cost, and analytical difficulties. We adopted a strategy that consisted of administering three sets of interviews. The first set was administered in person to Corpsmembers at centers and to the comparison sample in their homes. The purpose of the first interview was to collect baseline data on the pre-enrollment period for the Job Corps sample and similar data on the same period for the comparison sample. The timing of these interviews represents a compromise between minimizing the length of the recall period and maximizing the length of the observation period.

The first follow-up interviews were administered in person approximately nine months after the baseline. The entire comparison sample and a subset of the Job Corps sample were reinterviewed. The subset of the Job Corps group included all members of the original sample who had terminated from the program at least five months prior to the first follow-up interview (an effective cut-off date of leaving Job Corps by October 15, 1977). This criterion ensured an adequate period of postprogram observation within the constraints of the overall budget and the time permitted for the First Follow-Up Report (Mallar et al., 1978). The first follow-up sample of Corpsmembers had been out of the program for a time ranging from 5 to 9 months and 7 months on average at the time of the survey.

For the Second Follow-Up Survey the Job Corps sample size was increased by extending the cut-off date of Job Corps termination from October 15, 1977, to April 15, 1978 (yielding reasonable sample sizes--including 1,462 additional Corpsmembers compared to first follow-up, and excluding only long-term stayers who had been over-represented at baseline). The second follow-up sample of Corpsmembers had been out of the program for up to two years and at least one full year and 18 months on average at the time of the survey.

The only major changes in survey procedures between the Second Follow-Up Survey and the two previous surveys were decisions to use a mixed-mode interviewing strategy and to switch the primary interviewing mode from in-person interviewing to telephone interviewing.[1] However, in order to reduce the nonresponse problems associated with conducting a telephone survey of a sample that contains a large proportion of disadvantaged youths, in-person interviews were also attempted for sample members who either could not be located by telephone or were not responsive to the telephone interview. The cost-efficient design resulted in all of the comparison-group sites and one-half of the Corpsmember sites being targeted for in-person interviews if telephone attempts were unsuccessful.

The comparative evaluation of benefits and costs required additional data. Data on program costs were provided by the national Job Corps office. These cost data were supplemented with information from Job Corps centers (on center expenditures that were not included in Job Corps' financial data) and the U.S. Office of Management and Budget (on federal administrative costs that were not included in the Job Corps' financial data). In addition, special studies were made of a random selection of Job Corps work projects to value the products and services provided.[2] Finally, dollar values for many of the benefits had to be imputed from secondary data sources.[3]

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[1] This survey process is described further in Technical Reports B and H. The desirability of telephone interviewing at the second and subsequent follow-up survey is documented in Technical Report H and is supported by the analysis of nonresponse (see Chapter X and Technical Report L).

[2] This work is described further in Technical Reports D, E, F, and K.

[3] See Chapter V and Technical Reports D and K for more details.

III. OVERALL IMPACTS ON EMPLOYMENT AND RELATED ACTIVITIES

One of the primary goals of Job Corps is to help participants improve their lifetime economic prospects. The hypothesized economic impacts of Job Corps outlined in Chapter II are generally consistent with that goal. In this chapter we present overall findings on whether Job Corps is successfully fulfilling the hypothesized effects and achieving its goal of improving Corpsmembers' economic prospects during the first two years after Corpsmembers leave the program. More specifically, we provide empirical evidence on whether Job Corps is accomplishing the desired effects of (1) increasing employment and earnings, (2) improving future labor-market opportunities through work experience, education, training, better health, geographic mobility, and military service, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality. Furthermore, in this chapter we concentrate on the overall impacts of Job Corps and their occurrence and timing during the first two years after Corpsmembers leave the program (only the breakdowns required for estimation purposes are shown and discussed here--that is, by sex and program-completion categories).[1]

We begin this chapter with a brief discussion of the estimation procedures used. Detailed findings are then presented for each of the four desired impacts listed above.

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[1] In Chapter VI we present a much more thorough analysis of the differential impacts of Job Corps among Corpsmembers and Centers, including the differential effects by sex and program-completion categories.

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A. ECONOMETRIC PROCEDURES[1]

The data from the Second Follow-Up Survey should enable us to obtain more accurate estimates of Job Corps effects than was previously possible, both because the postprogram observation period was extended and because additional Corpsmembers were interviewed. At the time of the First Follow-Up Survey, youths in the Corpsmember sample had been out of Job Corps only from 5 to 9 months--approximately 7 months on average. By the time of the Second Follow-Up Survey, however, they had been out of the program from 1 to 2 years, with an average of slightly over 18 months. Further, in the Second Follow-Up Survey, postprogram interviews were attempted for 1,462 youths in the Corpsmember sample who had not been out of Job Corps long enough to be interviewed productively at the time of the First Follow-Up Survey.

The full panel of postprogram observations--incorporating data from both the First and Second Follow-Up surveys--was organized into quarterly aggregates for each youth in the sample. For example, the employment variable was defined as the percentage of the quarter employed, and was constructed for each individual youth for each quarterly time period in which any data were available. The data were arrayed into quarterly aggregates by calendar quarters according to the seasons--summer (June, July, and August), fall (September, October, and November), winter (December, January, and February), and spring (March, April, and May)--which differ from the usual fiscal quarters but provide better controls for seasonality. The first quarter for which we obtained postprogram data for any youth was spring 1977, and the last quarter was spring 1979.

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[1] For more details, see the appendix to this chapter and Chapter X.

Therefore, for each youth we have up to nine quarterly observations in the postprogram period that can be pooled in the statistical analysis, and the average is approximately seven quarterly observations, including partial quarters.

1. Regression Approach to Adjust for Differences Between the Job Corps and Comparison Groups

As discussed previously (see Mallar et al., 1978, Chapter IV), before-after differences for participants are inadequate measures of program impacts for youths and individuals whose pre-enrollment behavior reflects a temporary disequilibrium, where substantial changes in behavior can be expected to take place in the absence of the program. Similarly, program/comparison-group differences in sample means are suspect, even for carefully designed sampling strategies, because unobserved differences can be present in the absence of random assignments.

Generally, before-after differences would greatly overstate the beneficial economic impacts of youth programs because youths are just beginning to enter labor markets and their economic prospects would improve substantially with age even in the absence of Job Corps. In contrast, both the direction and magnitude of bias with simple program/comparison-group differences would be completely unknown a priori. Youths with the greatest likelihood of participating in Job Corps could be dominated either by those who perform inordinately well in training programs (i.e., large benefits) or by those who would do especially poorly outside the program (i.e., low opportunity cost to reducing their effort in the regular labor market). Given these competing factors in self-selection into the program, therefore, youths who choose to participate in Job Corps could be more or less productive in unobserved dimensions than observationally similar participants.

Given the above considerations, it seems clear that a regression approach is needed to control for both observed and unobserved differences between the Job Corps and comparison groups. In our previous research we relied on a relatively simple regression adjustment that was based on the change over time in program-comparison differences in sample means (or, equivalently, the program-comparison differences in changes in sample means over time), and which showed that our basic findings were unchanged for a wide range of more and less complicated techniques based on different assumptions (see Mallar, 1979). However, the change-in-differences methodology assumes that if Corpsmembers had not gone into Job Corps the growth rates in the behavioral variables of interest would be the same on average for the Corpsmember and comparison groups--and such an assumption becomes less tenable as the length of the postprogram observation is increased. Also, with added observations (more degrees of freedom) and improvements in computational procedures, less restrictive techniques (which are more complex computationally) become practicable. Therefore, in the analysis for this report we use regression approaches that control for both observed and unobserved differences between the Job Corps and comparison groups, but which have less restrictive underlying assumptions than the techniques used previously.

The observed variables that are included in the regressions control for the following differences: age (5 variables); pre-enrollment education (3 variables); race/ethnicity (4 variables); pre-enrollment health (1 variable); seasonality (3 variables) and time trends (2 variables); and pre-enrollment experiences with employment (1 variable), welfare (1 variable), illegal activities (1 variable), and drug usage (2 variables).[1/]

[1/] In the appendix to this chapter we present examples of coefficient estimates for the control variables and briefly discuss the influences of these variables on youth employment and related activities.

In addition, we use an econometric methodology that has been developed to control for unobserved differences between samples--specifically in this application, to control for unobserved differences between youths in the Job Corps and the comparison groups (for example, unobserved differences in employability and work motivation). With the technique used to control for unobserved differences, the basic procedure involves modeling and estimating program participation and then including in the regression equations for the behavior of interest a control variable that is a function of the estimated probability of program participation.[1/]

With the econometric procedures outlined above we should obtain unbiased estimates of the impact of Job Corps on participant behavior.[2/] In principle, therefore, the estimates of Job Corps effects presented in this chapter are based on differences between groups of Corpsmembers and comparison youths that have similar compositions in terms of both unobserved and observed characteristics. These procedures should also enable us to obtain unbiased estimates of what Corpsmembers' activities would have been had they not participated in Job Corps (see Table III.1).

2. Disaggregations of Impact Estimates

While the focus of this chapter is on the overall effects of Job Corps on participant behavior, some disaggregations are needed in order to obtain accurate overall estimates, especially for cases in which the

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[1/] For further details on the properties and implementation of these econometric procedures, see Barnow, Cain, and Goldberger (1978), Heckman (1979), Maddala and Lee (1976), Mallar (1979), and the appendix to this chapter.

[2/] We are using "unbiased" here to mean asymptotically unbiased, and under usual assumptions the estimators have the large sample property of statistical consistency.

unweighted sample over-represents some segments of Corpsmembers and under-represents others. Two general types of disaggregations are undertaken: (1) separate estimates of regression equations for subgroups of the population that have completely different behavioral relationships for the activities of interest, and (2) decompositions of the program-treatment specification to capture hypothesized differentials in observed Job Corps impacts.

Subgroups of the Population. In general, we have pooled observations across individuals and time to take full advantage of the panel nature of the data (discussed further below). However, separate estimates are computed for three subgroups of youths, based on their personal characteristics: (1) males, (2) females who have no children present, and (3) females who have children living with them. Our previous research (see Mallar et al., 1978) found that the behavioral relationships of interest were substantially different for these three subgroups, based on statistical tests (Chow tests) for differences in parameters. With an appropriate specification, however, we found that observations on youths could be pooled together across other demographic classifications, such as age, race/ethnicity, and marital status.[1/]

Females were intentionally over-represented in the comparison group in order to increase the precision of separate estimates for females. However, the necessity of disaggregating the female subgroup by presence of children was not completely anticipated, and, unexpectedly, the comparison group over-represents females who have children living with them. Overall, for the postprogram observation period, the Job Corps sample is composed

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[1/] The primary differences in behavior for these latter subgroups can be captured with simple specifications (e.g., dummy variables for age, race/ethnicity, and marital status).

of approximately 70 percent males, 23 percent females without children, and 7 percent females with children present; the corresponding percentages for the comparison group are 48, 26, and 26 percent, respectively. The Job Corps proportions (70, 23, and 7 percent) are used for weighting separate estimates to obtain the overall estimates of Job Corps effects.

As discussed more thoroughly in Chapter IV, the female subgroups pose additional problems for analysis. First, we are only beginning to explore the impacts of Job Corps on the fertility of Corpswomen (timing of births, number of births, and illegitimacy rates), and further observation and research are needed on this topic. Potentially, some of the largest impacts of Job Corps on females' behavior could come from decreases in fertility (delayed timing of births, reduced number of births, and decreased illegitimacy), which would increase employability and reduce welfare dependence. Separate estimates based on the presence and absence of children completely miss the impacts of Job Corps on the family status under which former Corpswomen are observed.

The second problem in the analysis of impacts on females is the extremely small sample sizes and the concomitant instability of estimates for Corpswomen who have children living with them. For example, during the postprogram period we observe only fifteen females who were early dropouts from Job Corps and who had children living with them at the time of either one of the two follow-up surveys. Not surprisingly, for the subgroup of females with children the estimates of Job Corps impacts on employment and earnings fluctuate erratically around zero and are sensitive to changes in the specifications of the control variables in the regression equations (see further in Chapter IV). For the purposes of the overall estimates in the current chapter, we factor in zero impacts for females with children, rather than choose among erratic effects that in general are insignificantly different from zero in the statistical sense. Using zero effects for females with children causes little harm to the overall estimates in any case, because this group represents only 7 percent of the postprogram sample of Corpsmembers and would be dominated by the other 93 percent (i.e., males and females without children). In terms of the overall impacts, a zero estimate for females with children undoubtedly causes less bias than the omission of Job Corps impacts on whichever subgroup in which Corpswomen are observed (i.e., with or without children).

Program-Treatment Specifications. Two disaggregations of Job Corps statuses among participants are used to improve the overall accuracy and to explicate the findings: (1) separate estimates by completion category, and (2) interactions with the length of time since leaving the program. The program effects are expected to vary across completion categories and by length of time out of the program. Also, the postprogram sample has distributions that are unrepresentative of all Corpsmembers in both of the above dimensions (hence, which will require some reweighting), and among Corpsmembers the postprogram observation period is shorter the greater the length of stay is in the program.

As discussed briefly in Chapter II, our sample design over-represents program completers because youths who stay in Job Corps for a long period of time have a higher probability of being at centers at any point in time and, specifically, when the sample was drawn. The three program-completion categories used by Job Corps--program completers, partial completers, and early dropouts--are convenient because data are readily available on their actual proportions among all enrollees. However, there is not a perfect correlation between length of stay in Job Corps and these completion categories because, given the individualized and self-paced nature of Job Corps instruction, some youths can complete the program faster than others. A partial completer is defined as a Corpsmember who stays in the program for at least ninety days and who completes a segment of the program, but not the entire program. Early dropouts are defined as youths who terminate from Job Corps before the end of their first ninety days at a center and who do not complete any part of the program.

For fiscal year 1977 the proportions of all Job Corps enrollees who become program completers, partial completers, and early dropouts are approximately 30, 30, and 40 percent, respectively. In contrast, the proportions of program completers, partial completers, and early dropouts in our postprogram sample are approximately 56, 35, and 9 percent, respectively. Therefore, in order to obtain impact estimates that are applicable to the average for all Job Corps enrollees, we need to use our knowledge of the "correct" proportions by completion status to reweight the observations. Estimates are computed separately for program completers, partial completers, and early dropouts, and are then added together with weights of 0.30, 0.30, and 0.40, respectively.[1]

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[1] To obtain estimates that are representative of all Corpsmembers, we reweight the separate estimates by completion statuses as follows:

Estimated effect for All Enrollees =
0.30 (Estimated effect for Program Completers)
+ 0.30 (Estimated effect for Partial Completers)
+ 0.40 (Estimated effect for Early Dropouts).

In addition, note that the relationship between the unweighted estimate for the sample and the separate estimates by completion categories is as follows:

Unweighted Estimate for Sample =
0.56 (Estimated effect for Program Completers)
+ 0.35 (Estimated effect for Partial Completers)
+ 0.09 (Estimated effect for Early Dropouts),

which clearly shows how the unweighted estimate over-represents Corpsmembers who are completers. Because the estimated impacts of Job Corps are usually much larger for completers, the effect of the reweighting to obtain estimates that are representative of all enrollees (and, hence, giving completers less weight than in the sample) is to lower the overall estimates compared to the unweighted estimates.

As discussed further in Chapter VI, with the data available thus far, we have not been able to obtain reliable estimates that control for unobserved differences among Corpsmembers by completion categories, due to identification problems in modeling completion simultaneously with employment or other related behavior. Even though there may be some biases among completion categories, however, the estimates for overall impacts should be unbiased. (We know the "true" proportion in each category.) Furthermore, the evidence (discussed further below) supports the conclusion that the observed differences by completion category are at least in part attributable to a program effect with completing.[1]

Having quarterly data for up to two years of postprogram observation enables us to learn a great deal about the timing of impacts after Corpsmembers leave the program. Our examinations of the timing of effects has already been fruitful in identifying transition problems as Corpsmembers leave the centers and re-enter the regular labor market. Furthermore, with the Second Follow-Up data we will be able to begin testing the alleged quick "fadeout," or "decay," of Job Corps effects that have been supported previously with less rigorous techniques and less accurate data. Estimates of the interactions between completion categories and length of time out of Job Corps are also important, because we have fewer observations as the length of the postprogram period increases, and because the observations that we do have for the longest period are for youths who had shorter stays in Job Corps on the average.

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[1] Briefly, we obtain statistically significant and moderate-sized effects for the program as a whole; the estimated effects for the group with near zero treatment (early dropouts) are close to zero; we control for a wide range of important variables that are observed; and the potential sources of remaining bias work in opposite directions.

We pool all of the quarterly observations for each individual youth and estimate two types of specifications by length of time out of the program--(1) 6-month averages (four variables for each completion category), and (2) a more flexible continuous time pattern (eleven variables for each completion category).[1] The 6-month averages are presented in tables, discussed extensively in the text, and form the basis for the benefit-cost estimates in Chapter V. The more continuous time patterns are presented in figures, and provide the most comprehensive evidence both on the general timing of effects and, specifically, on the duration of Job Corps impacts (how long they are maintained, how quickly they fade out, or how much further they grow).

B. EMPLOYMENT AND EARNINGS

One of the most important goals of Job Corps is to increase the employability of participating youths. As shown in Table III.1, the economic prospects for these youths could be expected to improve somewhat in the postprogram period as they age, even if they had not participated in Job Corps--especially when compared to their disadvantaged statuses in the pre-enrollment period (see Kerachsky and Mallar, 1978).[2] On an absolute scale, however, the economic prospects for Corpsmembers are not very good if they do not enter the program. For example, even by

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[1] Correlations of individual errors over time are adjusted in a two-stage error-components (or variance-components) model that should yield greater efficiency than ordinary least squares (for more details, see Maddala, 1971; Nerlove, 1971a and 1971b; and Wallace and Hussain, 1969). The computational program used enables us to include individuals with varying lengths of time (essential for our application) and allows individuals to be missing periods of data (early, late, or intervening quarters). For documentation of the computer program, see Avery (1975). As noted above, seasonality and time trends across individuals are specified explicitly in the regression equations.

[2] The trend over the two-year postprogram period in Table III.1 is attenuated for the time period of 18 to 24 months after termination, because the oldest Corpsmembers who were in Job Corps for a long time had yet to be observed for this time period; hence, they could not be included.

TABLE III.1

ESTIMATES OF CORPSMEMBERS' ACTIVITIES HAD THEY NOT PARTICIPATED IN JOB CORPS

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
A. Employment and earnings of civilians				
• Employed (fraction of time)	0.354	0.412	0.471	0.453
• Hours worked per week	13.13	15.84	18.38	17.73
• Earnings per week (dollars)	43.82	58.38	72.48	73.73
• Employed, in school, or in training (fraction of time)	0.470	0.519	0.562	0.555
• Employed or looking for work (fraction of time)	0.653	0.682	0.701	0.715
• Employed in union job (fraction of time)	0.063	0.070	0.083	0.082
• Employed in PSE job (fraction of time)	0.044	0.051	0.052	0.042
B. Probability in military during interview week	0.033	N.A.	0.051	N.A.
C. Education and training of civilians				
• Probability of having high school diploma or GED by time of interview	0.151	N.A.	0.109	N.A.
• In any school (fraction of time)	0.117	0.104	0.093	0.096
• In college (fraction of time)	<0.001	<0.001	<0.001	0.002
• In high school (fraction of time)	0.083	0.073	0.062	0.056
• In vocational or technical school (fraction of time)	0.002	0.001	0.005	0.005
• In other school (fraction of time)	0.038	0.033	0.030	0.034
• In any training program (fraction of time)	0.036	0.035	0.036	0.035
• In CETA training (fraction of time)	0.027	0.030	0.031	0.030
• In WIN training (fraction of time)	0.003	0.003	0.004	0.005
• In other training (fraction of time)	0.003	0.001	<0.001	<0.001
D. Number of moves for civilians (mobility)				
• All moves across cities (cumulative)	0.089	N.A.	0.240	N.A.
• For job opportunities (cumulative)	0.024	N.A.	0.229	N.A.
• For education or training (cumulative)	0.012	N.A.	0.067	N.A.
• All moves across cities excluding Job Corps relocations (cumulative)	0.081	N.A.	0.194	N.A.
E. Serious health problems for civilians (fraction of time)	0.036	0.042	0.040	0.045
F. Receipt of public assistance by civilians				
• Any financial assistance (fraction of time)	0.094	0.112	0.120	0.108
• AFDC (fraction of time)	0.059	0.079	0.084	0.078
• General Assistance or other (fraction of time)	0.036	0.034	0.034	0.033
• Food Stamps (fraction of time)	0.184	0.188	0.186	0.169
• Public housing (fraction of time)	0.079	0.105	0.098	0.094
G. Receipt of other transfers by civilians				
• Unemployment Insurance (fraction of time)	0.025	0.031	0.035	0.040
• Workers' Compensation (fraction of time)	0.005	0.002	0.007	0.006
• Training allowances (per six months in dollars)	24.55	15.59	10.99	10.71
H. Criminality				
• Total number of civilian arrests per six months	0.092	0.075	0.078	0.081
• Number of civilian theft arrests per six months	0.056	0.047	0.046	0.051
• Probability in jail during survey week	0.022	N.A.	0.011	N.A.

N.A. - Not Applicable

[a] The entries in this table are the appropriately weighted sample means observed for Job Corps participants minus the estimated effects shown in subsequent tables (Tables III.2 to III.6). Most of the variables are estimated for the civilian population only (see the notes for Tables III.2 to III.6 for more in the variable definitions and computations).

the end of the postprogram observation period their average annual earnings would be well under $4,000 ($73.73 x 52 = $3,833.96), only between 10 and 15 percent would have had a high school diploma or its equivalent, almost none of them would have attended college, and many would have been receiving public transfers (see Table III.1 for more details).[1]

In general, we find increasingly positive employment and earnings effects from Job Corps participation over the course of the first year of postprogram observation, as well as positive, stable, and statistically significant overall impacts for Corpsmembers during the second year of postprogram observation. As summarized in Table III.2, the estimated overall effects during their second year after leaving the program show statistically significant gains in employment among civilian Corpsmembers of over 8 percentage points, or over four weeks per year (the estimates of impacts on weeks worked shown in Table III.2 are for 6-month periods).[2] The corresponding earnings gain during the second postprogram year is approximately $500 on average for civilian Corpsmembers and is marginally significant for statistical tests against zero.[3]

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[1] Variables and impact estimates are often presented in "fraction of time" units throughout this chapter. These can easily be converted into discrete time units through simply multiplying by the appropriate factor. For example, to convert the numbers to weeks in a six-month period, simply multiply by 26; to convert the numbers to months in a six-month period, simply multiply by 6.

[2] To obtain observed sample means for Corpsmembers, the program effects (as in Table III.2) would be added to the values had they not participated in Job Corps (the values are given in Table III.1).

[3] Larger confidence levels than normal for two-tailed statistical tests and the one-tailed test equivalents are shown in all of our tables, both to provide additional information in a convenient format and because it can be argued that one-tailed tests are appropriate since participation in Job Corps is not expected to have deleterious effects on behavior.

TABLE III.2

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT AND EARNINGS[a]

Variable	Unweighted Postprogram Sample Mean[b]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES					
1. Civilians employment and earnings					
• Employed (fraction of time)	0.546	0.008	0.054	0.079*	0.114****
• Hours worked per week	21.74	1.52	2.51*	3.47**	4.82***
• Earnings per week (dollars)	82.81	-0.33	5.34	8.63	8.36
• Weeks worked per six months	14.20	0.22	1.39	2.05**	2.97****
• Employed, in school, or in training (fraction of time)	0,594	-0.042	0.013	0.024	0.063*
• Employed or looking for work (fraction of time)	0.792	-0.017	0.008	0.017	0.034
• Employed in union job (fraction of time)	0.075	-0.035	-0.028	-0.033	-0.030
• Employed in PSE job (fraction of time)	0.054	-0.024	-0.007	0.001	0.010
2. Probability in military during interview week[c]	0.101	0.024**	N.A.	0.056***	N.A.
B. FEMALES WITHOUT CHILDREN					
1. Civilians employment and earnings					
• Employed (fraction of time)	0.392	0.027	0.057	0.099**	0.045
• Hours worked per week	14.05	1.16	0.95	3.23*	1.22
• Earnings per week (dollars)	45.52	9.79	9.86	15.86**	13.79*
• Weeks worked per six months	10.20	0.070	1.48	2.57**	1.18
• Employed, in school, or in training (fraction of time)	0.490	-0.042	0.062	0.070	0.031
• Employed or looking for work (fraction of time)	0.620	0.179****	0.121***	0.146****	0.107**
• Employed in union job (fraction of time)	0.042	0.012	0.022	0.032	0.048**
• Employed in PSE job (fraction of time)	0.040	0.014	0.014	0.040**	-0.002
2. Probability in military during interview week[c]	0.027	0.018	N.A.	0.0003	N.A.

TABLE III.2 (continued)

Variable	Unweighted Postprogram Sample Mean[b]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
		C. OVERALL			
1. Civilians employment and earnings					
• Employed (fraction of time)	0.472	0.012	0.051*	0.078***	0.090****
• Hours worked per week	18.45	1.33	1.98*	3.17***	3.66***
• Earnings per week (dollars)	68.44	2.02	6.00	9.69*	9.03*
• Weeks worked per six months	12.29	0.315	1.31*	2.02****	2.35****
• Employed, in school, or in training (fraction of time)	0.529	-0.039	0.023	0.033	0.051*
• Employed or looking for work (fraction of time)	0.697	0.029	0.034	0.046*	0.048*
• Employed in union job (fraction of time)	0.062	-0.022	-0.015	-0.016	-0.010
• Employed in PSE job (fraction of time)	0.047	-0.014	-0.002	0.010	0.007
2. Probability in military during interview week[c]	0.077	0.019***	N.A.	0.039***	N.A.

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] Most of the variables in this table are estimated for the civilian population only. The one exception is for the probability of being in the military during the interview week.

[b] The unweighted postprogram sample means indicate the magnitude of the variables for all observations (Job Corps and comparison groups). Corpsmember sample means can be obtained by adding the effects shown in this table to the estimates of Corpsmembers' activities had they not participated in Job Corps (presented in Table III.1).

[c] The estimates for Job Corps effect on the probability of being in the military are based on probit estimates with data from the interview week for the two follow-up surveys. The estimates with the data from the First Follow-Up Survey are given in the "0 to 6 Months" column, and those from the Second Follow-Up Survey are given in the "12 to 18 Months" column. The unweighted postprogram sample mean given here is from the Second Follow-Up data.

For two reasons, the estimates presented in Table III.2 for employment and earnings gains among civilians understate the overall impacts of Job Corps on employment and earnings. First, Corpsmembers have some periods of re-enrollment in the program during our "postprogram" period, some of which are included with values of zero for employment, earnings, and all other activities. Second, we find substantial Job Corps effects on the probability of being in the military service (large and statistically significant).[1] The incidence of Job Corps re-enrollments being included with zero values is relatively small (less than 1 percent of our observations) and does not have a major effect on the size or statistical significance of the estimated impacts (5 percent reductions in effects at an extreme maximum). However, the inclusion of Job Corps impacts on military service substantially increases the size and significance of the estimated gain in participants' employment and earnings.

At the time of the Second Follow-Up Survey (approximately 18 months after termination from Job Corps on average), we find nearly a 4 percentage point increase in military service, so that, altogether, approximately 9 percent of former Corpsmembers are in the military service (base of 5.1 percent plus estimated impact of 3.9 percent yields 9.0 percent altogether). With the estimated military effects added to those for employment during the second postprogram year, we find a 9.7 percentage point increase in employment overall (i.e., 0.076 attributable to civilian employment plus 0.021 attributable to youths in the military yields 0.097, or 9.7 percent, altogether),[1] and a more than 5-week increase in annual employment (0.097 x 52 ≐ 5.04). Similarly, assuming that the youths in the military earn $8,000 per year on average,[2] the overall gain in earnings for the second postprogram year would be approximately $606.66 (i.e., $442.92 attributable to civilian employment plus $163.74 attributable to military service yields $606.66 altogether,[3] compared to $486.72 for civilians).[4]

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[1] Our description of the econometric procedures used as regression "approaches" and regression "equations" are meant in the general sense of the terms. In fact, all the probability models with binary dependent variables (e.g., the probabilities of military service, of having a high school diploma or GED, and of being in jail) were estimated with probit maximum likelihood techniques.

Other Job Corps effects presented in Table III.2 are for civilian activity rates (employment plus related activities), employment in union jobs among civilians, special public service employment (PSE) jobs under CETA for civilians, and disaggregations for males and females without children. By the end of the postprogram period, there are moderate-sized (approximately 5 percentage points) increases in civilian activity rates of marginal statistical significance, as evidenced by both the fraction of time employed, in school, or in a training or work-experience program and the fraction of time employed or looking for work (an approximation to the usual definition of labor-force participation). There are virtually no effects on either union employment or PSE employment. Finally, the Job Corps impacts on employment and earnings are similar for males and females, except for slightly larger gains among civilians for Corpswomen and much larger gains in military service for Corpsmen.

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[1] (0.910) (0.5) (0.078 + 0.090) = 0.076; (0.039) [1.0 - (0.5) (0.471 + 0.453)] ≐ 0.021; and 0.076 + 0.021 = 0.097.

[2] More careful valuations of military gains and estimates of fringe-benefit increases are developed and computed for the benefit-cost analysis in Chapter V.

[3] (0.910) (0.5) (52) ($9.69 + $9.03) ≐ $442.92; (0.039) [$8,000 - (0.5) (52) ($72.48 + $73.73)] ≐ $163.74; and $442.92 + $163.74 = $606.66.

[4] (0.5) (52) ($9.69 + $9.03) ≐ $486.72.

Figures III.1 through III.4 provide more details on the timing of Job Corps impacts on participants' employment and earnings during the postprogram period. The findings for the early postprogram period are generally consistent with those reported earlier (see Mallar et al., 1978). After some initial downfall during the transition from center life to the regular labor market (and after having been out of the regular labor market from a few days for early dropouts to up to two years for program completers), the Job Corps effects on employment and earnings become increasingly positive over time. The estimates average out to near zero for the first 6 months of postprogram observation and then become positive (see Table III.2, also). (The sources of Job Corps impacts still appear to stem from those other than job placement.) Furthermore, the current findings are very similar to those reported earlier. (Any small differences are primarily attributable to the greater precision in the current estimates due to added observations and a longer observation period to help control for spurious, non-Job Corps influences.)

The most interesting new finding from the extended postprogram observation period is the relatively stable estimates of employment and earnings gains among civilians, especially program completers, for months 6 to 20 in the postprogram period.[1/] There is no evidence of a deterioration in impacts over this period--in fact, if the increasing military gains were incorporated, we would show substantial growth in program effects on employment and earnings up to the end of postprogram observation period. We find no support for the fadeout that was previously inferred (see

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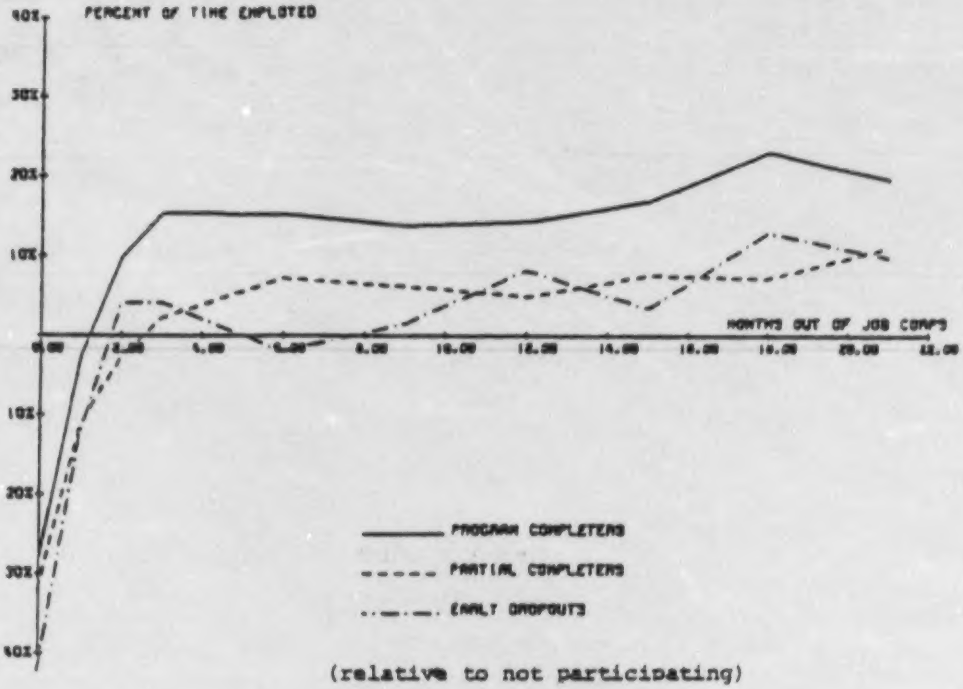
[1/] The figures end at month 21 of postprogram observation, because we have too few observations to provide reliable estimates beyond that point. The more erratic fluctuations for the early dropout group can be attributed to the small number of observations.

FIGURE III.1

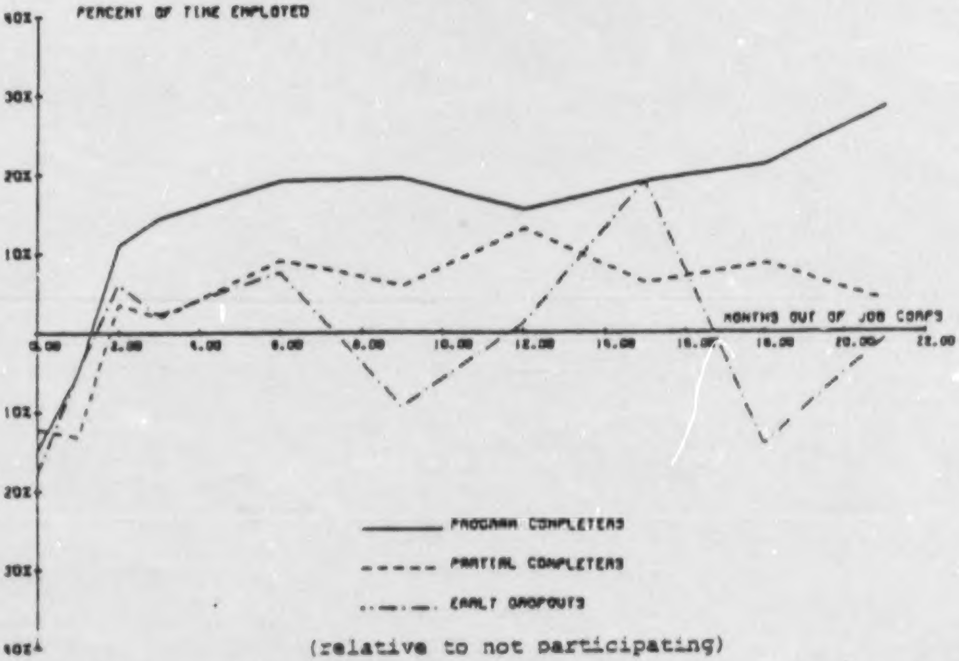
ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED FOR MALES



(relative to not participating)

FIGURE III.2

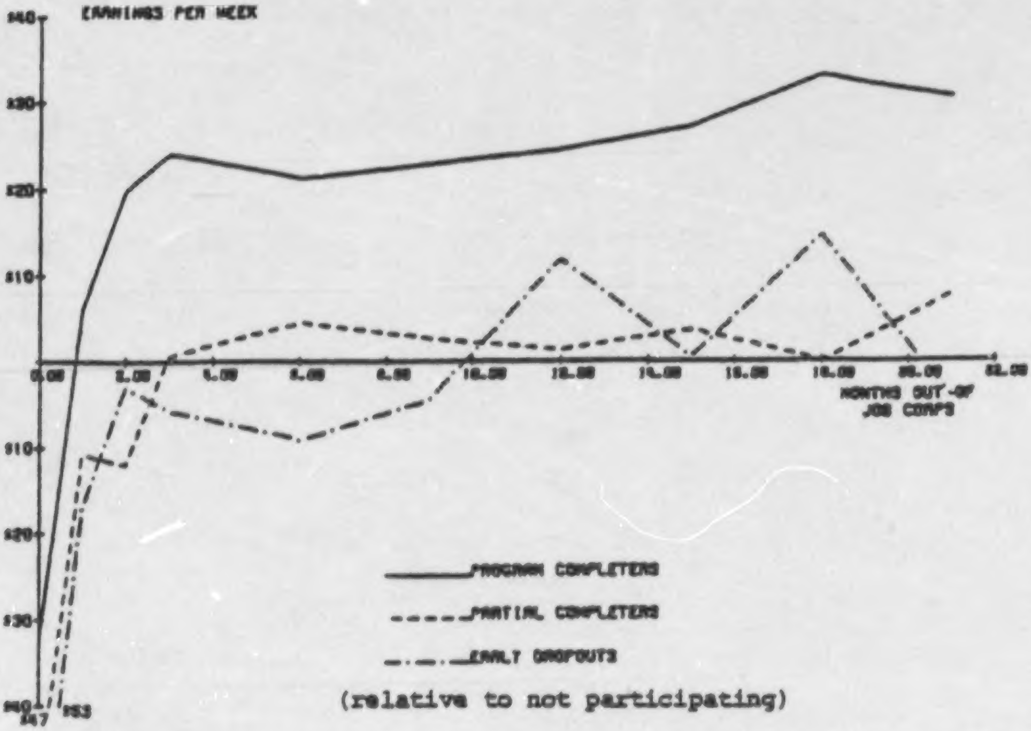
ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME EMPLOYED FOR FEMALES WITHOUT CHILDREN



(relative to not participating)

FIGURE III.3

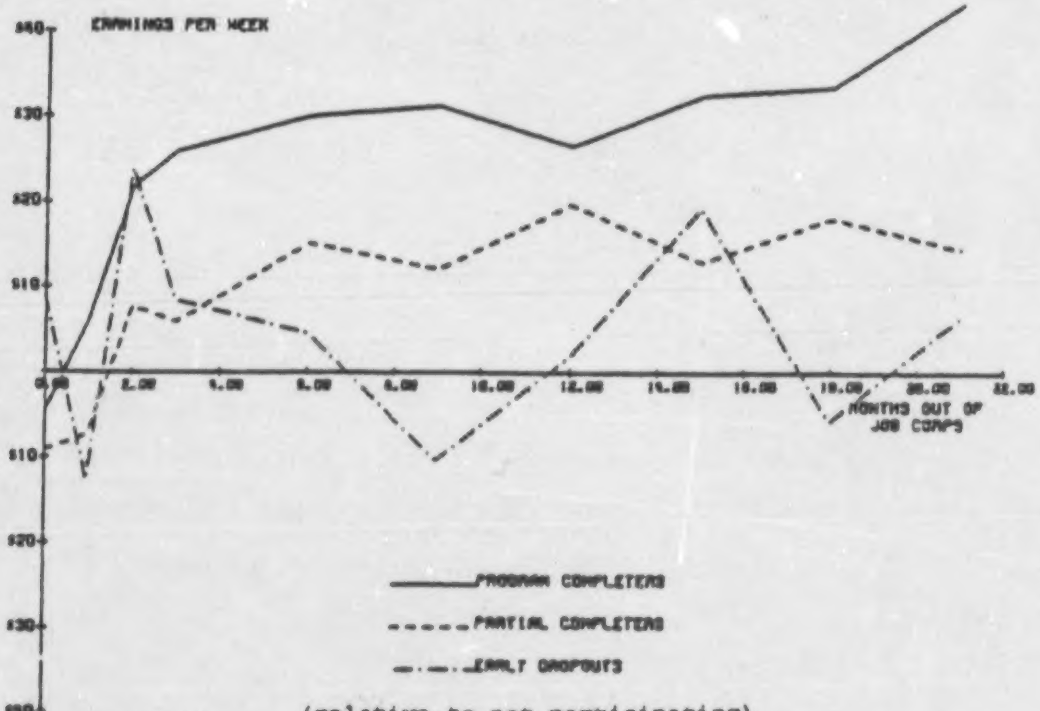
ESTIMATES OF TIME PATHS OF NET INCREASES IN EARNINGS FOR MALES



(relative to not participating)

FIGURE III.4

ESTIMATES OF TIME PATHS OF NET INCREASES IN EARNINGS FOR FEMALES WITHOUT CHILDREN



(relative to not participating)

Goldstein, 1972) from comparisons of Cain's (1968) 6-month findings to Woltman and Walton's (1968) 18-month findings. This is not particularly surprising, since the program has undoubtedly changed, and since both of these earlier studies were based on much less adequate data that prevented researchers from undertaking the kind of rigorous statistical analysis which underlies our findings.

The data in Figures III.1 through III.4 also show the differences by completion category that will be examined in more detail in Chapter VI. Underlying our moderate overall effects are very large gains in employment and earnings for program completers, and modest to zero gains for Corpsmembers who drop out before finishing the program. In general, aside from program completers, only female Corpsmembers who partially complete the program show statistically significant increases in employability.

Before leaving our discussion of the impacts of Job Corps on employment and earnings, some potential anomalies lurking behind the numbers need to be highlighted. First, from the estimated effects on employment and earnings, we can infer that there are little if any gains in hourly wage rates. However, this could be explained by one of three factors: (1) the inclusion of otherwise less able youths among employed Corpsmembers, with the increases in employment and military service, (2) the failure to include the gains in earnings among youths in the military service, and (3) effective constraints in youth labor markets, such as minimum-wage legislation, that tend to equalize observed hourly wage rates at young ages.

Second, while the data from the week prior to the Second Follow-Up Survey are generally consistent with the findings reported and discussed in this section, there is one notable exception--more negative earnings effects for males who did not finish the Job Corps program. This could be a spurious phenomenon (perhaps even causing downward bias in all of the estimated impacts on earnings through the positive correlation of current fluctuations and recall errors, sometimes referred to as "telescoping"), or the exception could be indicative of a quicker and more substantial fadeout of effects in the postprogram period, just beyond what we have observed so far.

Finally, in Chapter X we address other issues that affect the generalizability of the findings, such as education and training received by comparison group members (and, hence, who are obtaining program treatments of sorts) and the effects of survey nonresponse on the accuracy of estimates. However, as will be discussed in Chapter X, both of these potential problems in generalizing our findings appear, if anything, to cause us to underestimate the impacts of Job Corps on the employability of former participants.

C. INVESTMENTS IN HUMAN CAPITAL

Another goal of Job Corps is to increase participants' future, as well as short-term, employability (indirect program effects on productivity and earnings). Current activities that lead to future increases in productivity, employment, and earnings are defined in economics literature as "investments in human capital." Work experience is one type of activity that normally leads to increased productivity and employability in the future. Therefore, the short-term increases in employment and earnings for former Corpsmembers discussed in the previous section are also indicators of positive effects on investments in human capital. In this section we examine Job Corps impacts on participant education, training, and other activities that could also potentially lead to future gains in employment and earnings among Corpsmembers.

1. Education and Training of Civilians

As discussed above in Chapter II, the a priori basis for expecting increased postprogram investments in human capital for Corpsmembers in terms of education and training is more ambiguous than for employability and other forms of investments in human capital, for at least two reasons. First, education and training are provided in Job Corps, reducing both the need for and returns to postprogram education and training. Second, increased employability in the short-term provides incentives to engage in work activities which are alternatives to further education and training.

The findings for Job Corps effects on education and training are presented in Table III.3. They show (1) a very large and statistically significant increase in the probability of having a high school diploma or equivalent degree (especially when all of the General Educational Development, GED, degrees obtained through Job Corps study are included in the Second Follow-Up data); (2) moderate-sized and statistically significant increases in college attendance and decreases in high school attendance; and (3) small but marginally significant reductions in training. There are also small and marginally significant increases in enrollments in vocational and technical schools that are almost exactly matched by reduced enrollments in other miscellaneous educational programs (mostly adult education courses).

Overall, by the Second Follow-Up Survey there is a 25 percentage point increase in the probability that Corpsmembers have a high school diploma, GED, or equivalent degree. The large increase in high school, GED, and equivalent degrees among Corpsmembers more than explains the approximately 5 percentage-point reduction in high school attendance. College attendance shows a statistically significant increase of from 2 to 3 percentage points among Corpsmembers, or the equivalent of an

TABLE III.3

ESTIMATES OF JOB CORPS EFFECTS ON EDUCATION AND TRAINING OF CIVILIANS

Variable	Unweighted Postprogram Sample Mean[a/]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES					
1. Probability of having high school diploma or GED by time of interview[b/]	0.173	0.056****	N.A.	0.187****	N.A.
2. In any school (fraction of time)	0.058	-0.064****	-0.037*	-0.049***	-0.048***
• In college (fraction of time)	0.013	0.015	0.021**	0.025***	0.025***
• In high school (fraction of time)	0.021	-0.066****	-0.054****	-0.056****	-0.055****
• In vocational or technical school (fraction of time)	0.012	0.006	0.013	0.004	0.001
• In other school (fraction of time)	0.015	-0.020*	-0.017*	-0.022**	-0.020**
3. In any training program (fraction of time)	0.026	-0.014	-0.006	-0.017	-0.010
• In CETA training (fraction of time)	0.013	-0.021***	-0.019**	-0.022***	-0.011
• In WIN training (fraction of time)	0.0002	-0.001	-0.001	-0.002*	-0.001
• In other training (fraction of time)	0.005	-0.001	0.004	0.003	0.002
B. FEMALES WITHOUT CHILDREN					
1. Probability of having high school diploma or GED by time of interview[b/]	0.349	0.057****	N.A.	0.533****	N.A.
2. In any school (fraction of time)	0.109	-0.039	0.040	0.021	0.066**
• In college (fraction of time)	0.037	0.016	0.031*	0.031	0.054***
• In high school (fraction of time)	0.032	-0.054****	-0.031*	-0.046***	-0.048***
• In vocational or technical school (fraction of time)	0.024	0.021	0.045****	0.042***	0.059****
• In other school (fraction of time)	0.020	-0.027**	-0.009	-0.008	-0.0001
3. In any training program (fraction of time)	0.031	0.003	0.033**	0.010	-0.013
• In CETA training (fraction of time)	0.017	-0.003	0.009	-0.012	-0.024*
• In WIN training (fraction of time)	0.002	-0.008*	-0.011***	-0.013***	-0.014***
• In other training (fraction of time)	0.003	0.010*	0.025****	0.027****	0.010*

TABLE III.3 (continued)

Variable	Unweighted Postprogram Sample Mean[a]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
C. OVERALL					
1. Probability of having high school diploma or GED by time of interview[b]	0.201	0.052****	N.A.	0.250****	N.A.
2. In any school (fraction of time)	0.066	-0.054****	-0.017	-0.030*	-0.019
• In college (fraction of time)	0.018	0.014*	0.022***	0.025***	0.030****
• In high school (fraction of time)	0.022	-0.059****	-0.045****	-0.049****	-0.049****
• In vocational or technical school (fraction of time)	0.014	0.009	0.019***	0.012*	0.014*
• In other school (fraction of time)	0.015	-0.020***	-0.014*	-0.017**	-0.014**
3. In any training program (fraction of time)	0.025	-0.010	0.003	-0.010	-0.010
• In CETA training (fraction of time)	0.013	-0.015**	-0.011*	-0.018***	-0.013*
• In WIN training (fraction of time)	0.001	-0.002*	-0.003***	-0.004****	-0.004***
• In other training (fraction of time)	0.004	0.001	0.008*	0.008*	0.004

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] The unweighted postprogram sample means indicate the magnitude of the variables for all observations (Job Corps and comparison groups). Corpsmember sample means can be obtained by adding the effects shown in this table to the estimates for Corpsmembers' activities had they not participated in Job Corps (presented in Table III.1).

[b] The estimate for Job Corps effects on the probability of having a high school diploma or GED are based on probit estimates with data through the First Follow-Up Survey for estimates given in the "0 to 6 Months" column and with data through the Second Follow-Up Survey and including information from Job Corps termination forms for the estimates given in the "12 to 18 Months" column. (The estimates for the second period include all of the GEDs obtained in the Job Corps, while those for the first time period include only part of them.) The unweighted postprogram sample mean given here is from the data underlying the estimate for the "12 to 18 Months" column.

increase of nearly 5 full-time college students for every 100 youths enrolled in Job Corps. Therefore, the education affects appear to indicate some clear increases in human capital investments among Corpsmembers, and provide further evidence that the short-term gains in employability are not likely to fade out rapidly.

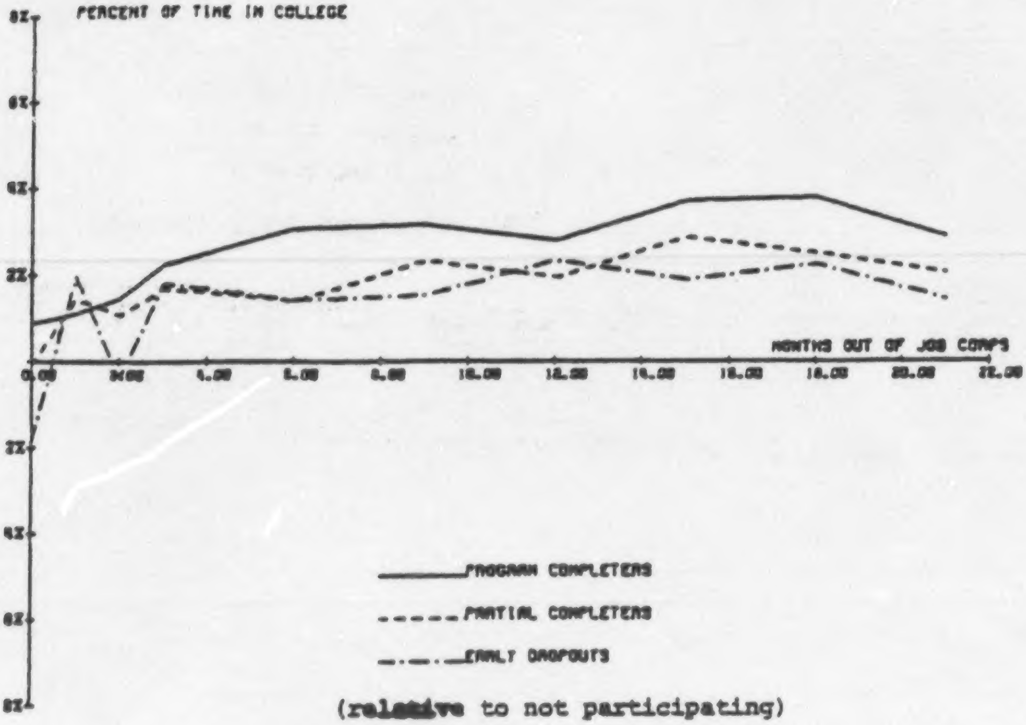
As shown in Table III.3 and Figures III.5 through III.8, the estimates of increased investments in human capital from education are slightly larger for females without children than for males. Furthermore, Figures III.5 through III.8 also show that the educational effects are relatively constant over time and across Job Corps completion categories, with only small increases in educational effects over the postprogram period and slightly higher college attendance among program completers.

The estimates of Job Corps effects on training show a very small (approximately 1 percentage point) reduction that is marginally significant and slightly larger in magnitude for females without children than for males. However, the effects for high school and training activities and effects do serve to indicate that our comparisons are consistent with the normal course of activities for youth--including some training and education that would have occurred if Corpsmembers had not gone into Job Corps--and do not constitute a "zero treatment" comparison (discussed further in Chapter X).

In summary, Job Corps-induced increases in human capital investments are evidenced by estimated increases in both high school equivalent degrees and attendance in higher education. Enrollments in lower levels of education and for training programs decline somewhat in the postprogram period.

FIGURE III.5

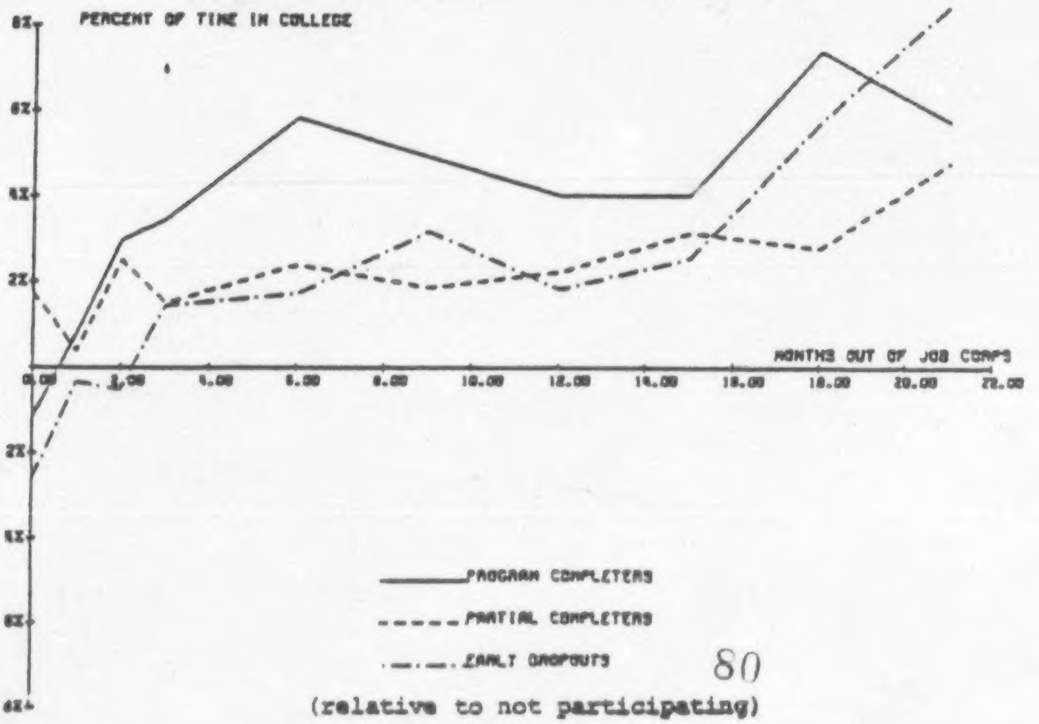
ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME IN COLLEGE FOR MALES



(relative to not participating)

FIGURE III.6

ESTIMATES OF TIME PATHS OF NET INCREASES IN PERCENT OF TIME IN COLLEGE FOR FEMALES WITHOUT CHILDREN



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(relative to not participating)

FIGURE III.7

ESTIMATES OF TIME PATHS OF NET REDUCTIONS IN PERCENT OF TIME IN HIGH SCHOOL FOR MALES

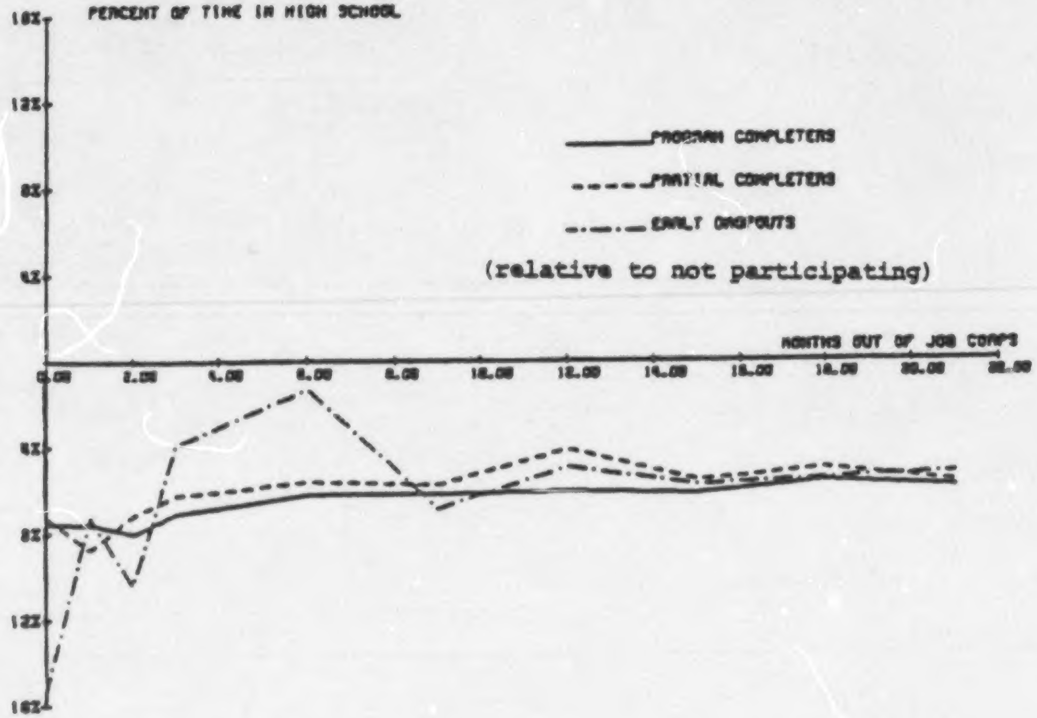
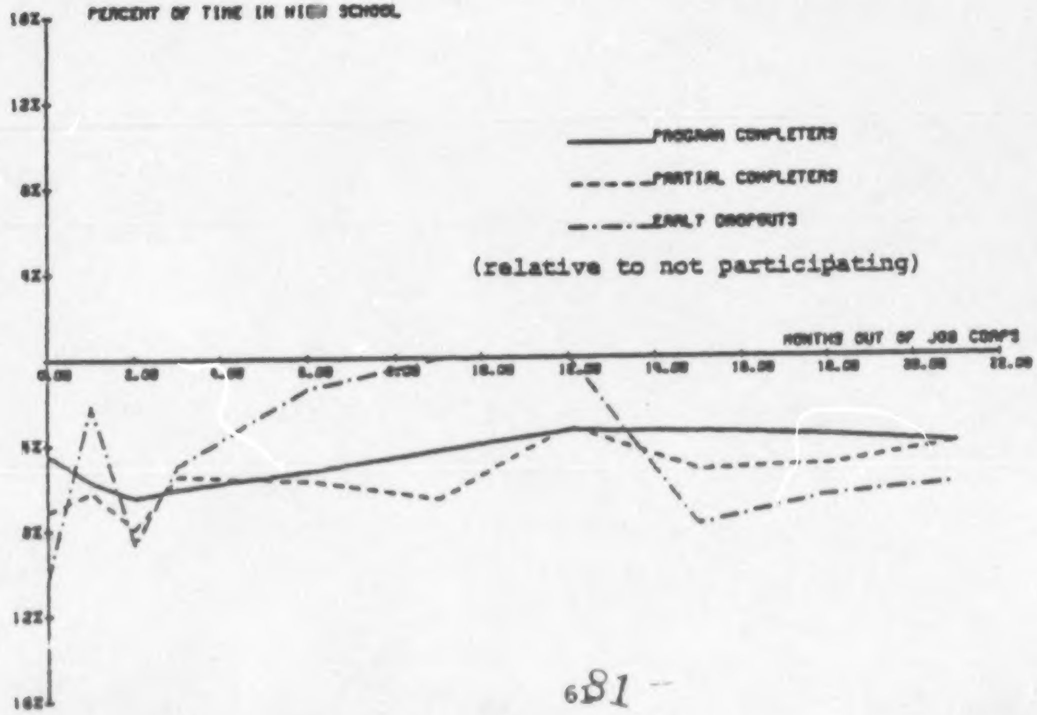


FIGURE III.8

ESTIMATES OF TIME PATHS OF NET REDUCTIONS IN PERCENT OF TIME IN HIGH SCHOOL FOR FEMALES WITHOUT CHILDREN



2. Other Investments in Human Capital

The findings on Job Corps effects for other types of investments in human capital are summarized in Table III.4. Corpsmembers had slightly better health, showed great mobility, and were more likely to have joined the military.

There are some overall reductions in serious health problems, but they are very small (approximately 1 percentage point) and statistically insignificant. However, the health gains are much larger (5 to 7 percentage points) and statistically significant for females without children. The overall effect on serious health problems shows only a slight decrease despite the findings for females without children, because there is a slight increase for males and they constitute the bulk (70 percent) of Job Corps enrollees.

The increases in mobility for Corpsmembers are evidenced by moves for job opportunities, for education or training, and otherwise. The overall impact estimates are substantial--altogether, over 20 additional moves for each 100 Corpsmembers--and statistically significant, but they occur primarily during the first 6 months after Corpsmembers leave the Centers. The cumulative impacts for moves do not change much from 6 to 18 months of postprogram observation. Furthermore, the Job Corps effects on moves for job opportunities and for education or training actually decline from month 6 to month 18 in the postprogram observation period--in part, no doubt, because Corpsmembers have increased employment at the end of 6 months that persists over the remainder of the postprogram observation period.

Finally, Table III.4 repeats the estimates of Job Corps impacts on military service. They can be viewed as investments in human capital, because increasing the ability of disadvantaged youths to pass exams to

TABLE III.4

ESTIMATES OF JOB CORPS EFFECTS ON HEALTH, MOBILITY, AND MILITARY SERVICE[a]

Variable	Unweighted Postprogram Sample Mean[b]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES					
1. Serious health problems for civilians (fraction of time)	0.027	0.006	0.010	0.012	0.005
2. Number of moves for civilians (mobility)[c]					
• All moves across cities (cumulative)	0.429	0.264****	N.A.	0.310***	N.A.
• For job opportunities (cumulative)	0.359	0.191****	N.A.	0.228**	N.A.
• For education or training (cumulative)	0.077	0.063****	N.A.	0.011	N.A.
• All moves across cities, excluding Job Corps relocations (cumulative)	0.359	0.177****	N.A.	0.253***	N.A.
3. Probability in military during interview week[d]	0.101	0.024**	N.A.	0.056***	N.A.
B. FEMALES WITHOUT CHILDREN					
1. Serious health problems for civilians (fraction of time)	0.035	-0.050***	-0.051***	-0.074***	-0.068***
2. Number of moves for civilians (mobility)[c]					
• All moves across cities (cumulative)	0.523	0.325***	N.A.	0.233	N.A.
• For job opportunities (cumulative)	0.260	0.195***	N.A.	-0.047	N.A.
• For education or training (cumulative)	0.090	0.050**	N.A.	0.098	N.A.
• All moves across cities, excluding Job Corps relocations (cumulative)	0.411	0.189****	N.A.	0.163	N.A.
3. Probability in military during interview week[d]	0.027	0.008	N.A.	0.0003	N.A.

TABLE III.4 (continued)

Variable	Unweighted Postprogram Sample Mean[b]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
		C. OVERALL			
1. Serious health problems for civilians (fraction of time)	0.027	-0.007	-0.005	-0.008	-0.012
2. Number of moves for civilians (mobility)[c]					
• All moves across cities (cumulative)	0.421	0.273****	N.A.	0.270***	N.A.
• For job opportunities (cumulative)	0.311	0.184****	N.A.	0.149*	N.A.
• For education or training (cumulative)	0.075	0.059****	N.A.	0.030	N.A.
• All moves across cities, excluding Job Corps relocations (cumulative)	0.346	0.174****	N.A.	0.215***	N.A.
3. Probability in military during interview week[d]	0.077	0.019***	N.A.	0.039***	N.A.

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] Most of the variables in this table are estimated for the civilian population only--the one exception is for the probability of being in the military during the interview week.

[b] The unweighted postprogram sample means indicate the magnitude of the variables for all observations (Job Corps and comparison groups). Corpsmember sample means can be obtained by adding the effects shown in this table to the estimates of Corpsmembers' activities had they not participated in Job Corps (presented in Table III.1).

[c] The estimates of Job Corps effects on the number of moves are based on data through the First Follow-Up Survey in the "0 to 6 Months" column and with data through the Second Follow-Up Survey in the "12 to 18 Months" column. The estimates in the "12 to 18" Month column are cumulative through the Second Follow-Up Survey. The unweighted postprogram sample mean given here is for the data through the Second Follow-Up.

[d] The estimates for Job Corps effects on the probability of being in the military are based on probit estimates with data from the interview week for the two follow-up surveys. The estimates with the data from the First Follow-Up Survey are given in the "0 to 6 Months" column, and those from the Second Follow-Up Survey are given in the "12 to 18 Months" column. The unweighted postprogram sample mean given here is from the Second Follow-Up data

enter the military and the concomitant increases in military service can be expected to cause an increase in the long-run earnings potential of these youths. While the gain in military service was also reported above as an employment effect, it does have human-capital implications because (1) entering the military (i.e., passing the Armed Forces Qualifying Examination) indicates the attainment of a certain level of human-capital development, and (2) participation in the military offers additional human-capital development through training and job experience. As already noted above, the increases in civilian employment should also produce some long-run human-capital benefits through job experience.

D. DEPENDENCE ON PUBLIC TRANSFERS

Overall, as shown in Table III.5, former Corpsmembers reduced their participation in all of the public transfer programs that were measured. We found estimated Job Corps effects of reductions in receipt of AFDC, General Assistance, Food Stamps, public housing, Unemployment Insurance, Workers' Compensation, and public training allowances. The largest and most significant reductions were for the regular cash assistance programs--AFDC, General Assistance, and Unemployment Insurance. The reductions in dependence on public assistance were larger in magnitude for females without children than for males, and the reductions in dependence on other transfer programs were larger for males than females. Finally, as shown in Figures III.9 and III.10 for AFDC and General Assistance, the reductions in welfare dependence are relatively constant over the postprogram period and are only slightly larger in magnitude for youths who complete the program than for those who drop out before finishing.

TABLE III.5

ESTIMATES OF JOB CORPS EFFECTS ON RECEIPT OF PUBLIC TRANSFERS BY CIVILIANS

Variable	Unweighted Postprogram Sample Mean	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES					
1. Receipt of public assistance					
• Any financial assistance (fraction of time)	0.017	-0.030***	-0.035****	-0.030***	-0.030***
• AFDC (fraction of time)	0.009	-0.002	-0.010	-0.010	-0.013
• General Assistance or other (fraction of time)	0.008	-0.028****	-0.025****	-0.020***	-0.018***
• Food Stamps (fraction of time)	0.134	-0.004	-0.005	0.004	0.011
• Public housing (fraction of time)	0.078	0.019	0.021	0.007	-0.009
2. Receipt of other transfers					
• Unemployment Insurance (fraction of time)[a]	0.014	-0.027****	-0.030****	-0.025****	-0.023****
• Workers' Compensation (fraction of time)[a]	0.004	-0.006	-0.003	-0.006	-0.004
• Training allowances (per six months in dollars)	11.64	-13.57	-9.54	-12.82	-11.50
B. FEMALES WITHOUT CHILDREN					
1. Receipt of public assistance					
• Any financial assistance (fraction of time)	0.043	-0.143****	-0.133****	-0.138****	-0.149****
• AFDC (fraction of time)	0.029	-0.114****	-0.102****	-0.110****	-0.118****
• General Assistance or other (fraction of time)	0.014	-0.030***	-0.030***	-0.027**	-0.030**
• Food Stamps (fraction of time)	0.156	-0.073*	-0.056	-0.042	-0.043
• Public housing (fraction of time)	0.070	-0.023	-0.032	-0.042	-0.046*
2. Receipt of other transfers					
• Unemployment Insurance (fraction of time)[a]	0.008	-0.015***	-0.018***	-0.019***	-0.018***
• Workers' Compensation (fraction of time)[a]	0.002	-0.001	-0.002	-0.002	-0.0003
• Training allowances (per six months in dollars)	8.16	12.44	24.51***	8.58	3.08

TABLE III.5 (continued)

Variable	Unweighted Postprogram Sample Mean	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
		C. OVERALL			
1. Receipt of public assistance					
• Any financial assistance (fraction of time)	0.022	-0.054****	-0.055****	-0.053****	-0.056****
• AFDC (fraction of time)	0.013	-0.028****	-0.031****	-0.033****	-0.036****
• General Assistance or other (fraction of time)	0.009	-0.027****	-0.025****	-0.021****	-0.020****
• Food Stamps (fraction of time)	0.130	-0.020	-0.016	-0.007	-0.002
• Public housing (fraction of time)	0.071	0.008	-0.008	-0.005	-0.017
2. Receipt of other transfers					
• Unemployment Insurance (fraction of time)[a]	0.012	-0.022****	-0.025****	-0.022****	-0.020****
• Workers' Compensation (fraction of time)[a]	0.003	-0.004	-0.002	-0.005	-0.003
• Training allowances (per six months in dollars)	10.02	-6.64	-1.04	-7.00	-7.34

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).

**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).

***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).

****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] The effects on Unemployment Insurance and Workers' Compensation were also estimated with aggregate data on the number of weeks received during the postprogram period (weeks are the basic time unit for participating in these programs). The resulting estimates showed slightly larger and more significant reductions. However, with estimates based on data aggregated over the whole postprogram period, the time patterns of effects could not be discerned.

FIGURE III.9

ESTIMATES OF TIME PATHS OF NET REDUCTIONS IN PERCENT OF TIME ON WELFARE FOR MALES

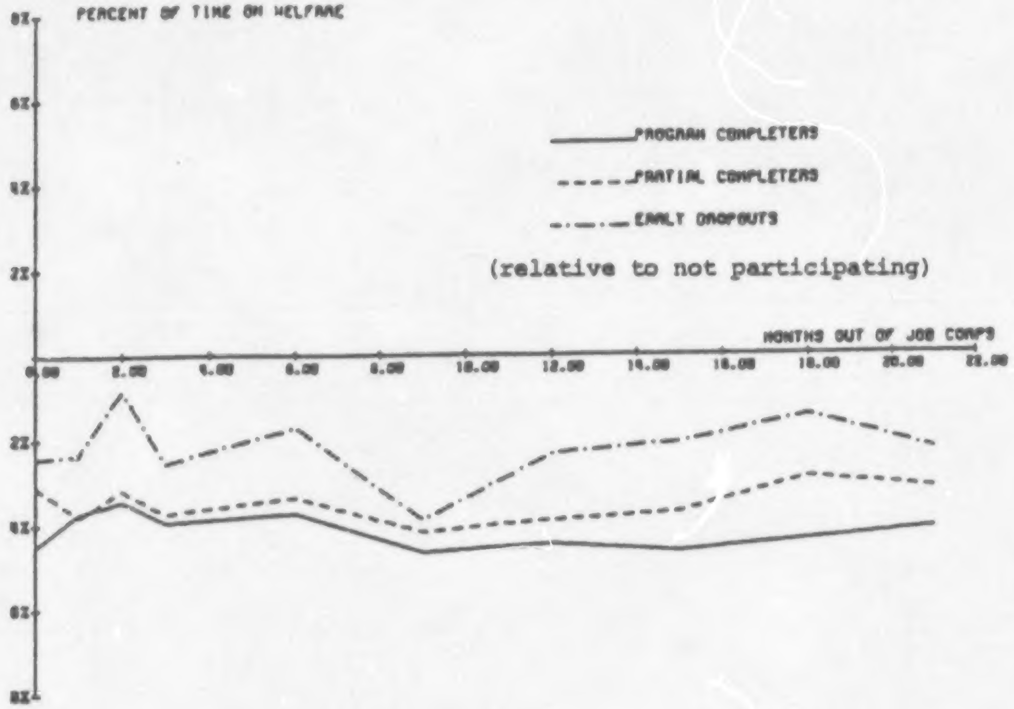
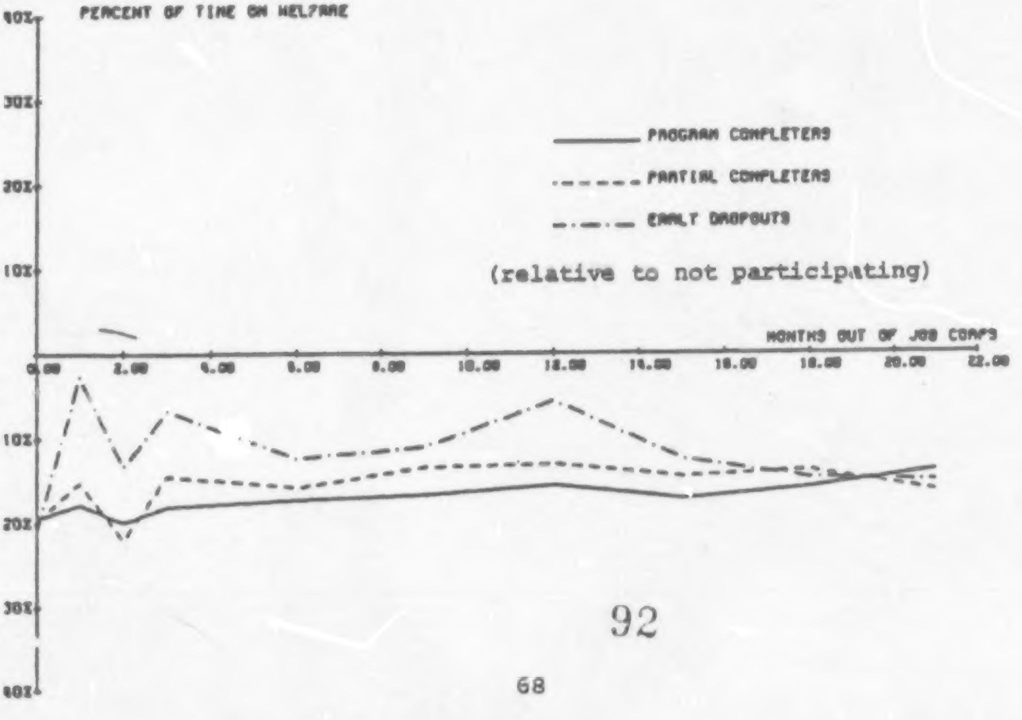


FIGURE III.10

ESTIMATES OF TIME PATHS OF NET REDUCTIONS IN PERCENT OF TIME ON WELFARE FOR FEMALES WITHOUT CHILDREN



E. CRIMINALITY

The reductions in criminality as measured by arrest estimates are the only area in which the increased observations, and the more rigorous statistical techniques that they afford, yield substantially smaller estimates of impacts during the first 6 postprogram months (about half as large in Table III.6, compared to those in Mallar et al., 1978). The initial arrest differences for our updated findings shown in Table III.6 are still relatively large and statistically significant. However, they fade out quickly during the second year of postprogram observation. This deterioration appears to be caused by some rather sudden shifts in the estimated effects for crimes other than thefts, and thus must be investigated further.

The differences by subgroup are also puzzling, and require further investigation. The reductions in arrests for males are exclusively among theft crimes, and the reductions for females are exclusively among crimes other than theft. The detailed composition of arrest effects and their social value will be examined further in Chapter V.

The effects of Job Corps on the probability of being in jail are again negligible and completely insignificant (almost none of the youths is in jail). The data from the postprogram observation period also support the earlier findings of small but statistically significant reductions in the use of drug-treatment programs among Corpsmembers (see Chapter V for more detailed breakdowns of drug-treatment effects).

F. SUMMARY AND CONCLUSIONS

The hypothesized economic impacts of Job Corps on participants' postprogram behavior are generally consistent with the program goal of improving Corpsmembers' economic prospects (see Chapter II). In this chapter, we have presented findings on the estimated postprogram effects

TABLE III.6

ESTIMATES OF JOB CORPS EFFECTS ON CRIMINALITY AND DRUG USAGE[a/]

Variable	Unweighted Postprogram Sample Mean	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
A. MALES					
1. Total number of civilian arrests per six months	0.079	-0.037	-0.033	-0.004	0.0003
2. Number of civilians theft arrests per six months	0.027	-0.041***	-0.041***	-0.027*	-0.047****
3. Probability in jail during survey week[b/]	0.044	-0.002	N.A.	0.027	N.A.
4. Proportion of time in drug treatment programs	0.004	N.A.	-0.008***	-0.008***	-0.008***
B. FEMALES WITHOUT CHILDREN					
1. Total number of civilian arrests per six months	0.009	-0.018*	-0.021**	-0.019*	0.016
2. Number of civilians theft arrests per six months	0.003	0.004	0.002	0.003	[illegible]
3. Probability in jail during survey week[b/]	0.0	0.005	N.A.	0.0	N.A.
4. Proportion of time in drug treatment programs	0.001	N.A.	0.0003	0.0004	0.002

TABLE III.6 (continued)

Variable	Unweighted Postprogram Sample Mean	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
C. OVERALL					
1. Total number of civilian arrests per six months	0.057	-0.030***	-0.028***	-0.008	0.004
2. Number of civilians theft arrests per six months	0.020	-0.028***	-0.028***	-0.018*	-0.033***
3. Probability in jail during survey week[b]	0.031	-0.0003	N.A.	0.019	N.A.
4. Proportion of time in drug treatment programs	0.003	N.A.	-0.006***	-0.005**	-0.005**

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).

**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).

***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).

****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Available (The questions in the First Follow-Up on receipt of drug treatment obtained information only on whether received anytime during the period and do not allow for estimates of the magnitude of usage as presented here. However, the earlier data did indicate reductions in receipt of drug treatments on the order of that shown here (see Mallar et al., 1978).

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] Most of the variables in this table are estimated for the civilian population only. The one exception is for the probability of being in jail during the survey week. More detailed estimates of arrest effects are presented with the benefit-cost analysis in Chapter V. The estimates in Chapter V include youths in the military as well as civilians, and are disaggregated by major crime categories.

[b] The estimates for Job Corps effects on the probability of being in jail are based on probit estimates with data from the interview week for the two follow-up surveys. The estimates with the data from the First Follow-Up Survey are given in the "0 to 6 Months" column, and those from the Second Follow-Up Survey are given in the "12 to 18 Months" column. The unweighted postprogram sample mean given here is from the Second Follow-Up data.

of Job Corps and have statistically tested the hypotheses and, hence, have tested how well the program appears to be meeting its goal of improving Corpsmembers' economic prospects during the first two years after Corpsmembers leave the program. Overall, we find that Job Corps is at least moderately successful in achieving its desired effects of (1) increasing employment and earnings, (2) improving future labor-market opportunities through work experience, education, training, better health, geographic mobility, and military service, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality. The estimates and their statistical significance are summarized in Table III.7; they are presented in an annualized basis in Table III.8; and finally, they are presented on a percentage basis in Table III.9.

The effects generally represent benefits and are statistically significant both overall and separately for males and females without children (exceptions are noted in the chapter). Also, the effects tend to be larger for program completers (see Chapter VI).

Beyond the overall positive results, the most noteworthy finding is that the impacts generally persist through the second year of postprogram observation. In fact, if there is any trend over the postprogram observation period, it appears to be toward increased program benefits during the first few months (especially for employability during the transition from center life to the regular labor market), and then stable effects through the rest of the two-year period. The one exception is for criminality, which shows its largest reductions during the early postprogram period and fades out rapidly after Corpsmembers have been out of the program for a year.

TABLE III.7

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS OF JOB CORPS[a/]

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
A. Employment and earnings of civilians				
• Employed (fraction of time)	0.012	0.051*	0.078***	0.090****
• Hours worked per week	1.33	1.98*	3.17***	3.66***
• Earnings per week (dollars)	2.02	6.00	9.69*	9.03*
• Employed, in school, or in training (fraction of time)	-0.039	0.023	0.033	0.051*
• Employed or looking for work (fraction of time)	0.029	0.034	0.046*	0.048*
• Employed in union job (fraction of time)	-0.022	-0.015	-0.016	-0.010
• Employed in PSE job (fraction of time)	-0.014	-0.002	0.010	0.007
B. Probability in military during interview week	0.019***	N.A.	0.039***	N.A.
C. Education and training of civilians				
• Probability of having high school diploma or GED by time of interview	0.052****	N.A.	0.250****	N.A.
• In any school (fraction of time)	-0.054****	-0.017	-0.030*	-0.019
• In college (fraction of time)	0.014*	0.022***	0.025***	0.030****
• In high school (fraction of time)	-0.059****	-0.045****	-0.049****	-0.049****
• In vocational or technical school (fraction of time)	0.009	0.019***	0.012*	0.014*
• In other school (fraction of time)	-0.020***	-0.014*	-0.017**	-0.014**
• In any training program (fraction of time)	-0.010	0.003	-0.010	-0.010
• In CETA training (fraction of time)	-0.015**	-0.011*	-0.018***	-0.013*
• In WIN training (fraction of time)	-0.002*	-0.003***	-0.004****	-0.004***
• In other training (fraction of time)	0.001	0.008*	0.008*	0.004
D. Number of moves for civilians (mobility)				
• All moves across cities (cumulative)	0.273****	N.A.	0.270***	N.A.
• For job opportunities (cumulative)	0.184****	N.A.	0.149*	N.A.
• For education or training (cumulative)	0.059****	N.A.	0.030	N.A.
• All moves across cities excluding Job Corps relocations (cumulative)	0.174****	N.A.	0.215***	N.A.

Table III.7 (continued)

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
E. Serious health problems for civilians (fraction of time)	-0.007	-0.005	-0.008	-0.012
F. Receipt of public assistance by civilians				
• Any financial assistance (fraction of time)	-0.054****	-0.055****	-0.053****	-0.056****
• AFDC (fraction of time)	-0.028****	-0.031****	-0.033****	-0.036****
• General Assistance or other (fraction of time)	-0.027****	-0.025****	-0.021****	-0.020****
• Food Stamps (fraction of time)	-0.020	-0.016	-0.007	-0.002
• Public housing (fraction of time)	0.008	-0.008	-0.005	-0.017
G. Receipt of other transfers by civilians				
• Unemployment Insurance (fraction of time)	-0.022****	-0.025****	-0.022****	-0.020****
• Workers' Compensation (fraction of time)	-0.004	-0.002	-0.005	-0.003
• Training allowances (per six months in dollars)	-6.64	-1.04	-7.00	-7.34
H. Criminality				
• Total number of civilian arrests per six months	-0.030***	-0.028***	-0.008	0.004
• Number of civilian theft arrests per six months	-0.028***	-0.028***	-0.018*	-0.033***
• Probability in jail during survey week	-0.0003	N.A.	0.019	N.A.

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] The entries in this table summarize the estimates presented in Tables III.2 to III.6. Most of the variables are estimated for the civilian population only (for more on the variable definitions and computations, see the notes for Tables III.2 to III.6).

Table III.8 (continued)

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
E. Serious health problems for civilians (weeks)	-0.38	-0.24	-0.44	-0.62
F. Receipt of public assistance by civilians				
• Any financial assistance (weeks)	-2.82****	-2.88****	-2.75****	-2.89****
• AFDC (weeks)	-1.45****	-1.60****	-1.70****	-1.87****
• General Assistance or other (weeks)	-1.39****	-1.28****	-1.09****	-1.02****
• Food Stamps (weeks)	-1.02	-0.84	-0.37	-0.10
• Public housing (weeks)	0.43	0.42	-0.26	-0.88
G. Receipt of other transfers by civilians				
• Unemployment Insurance (weeks)	-1.17****	-1.31****	-1.12****	-1.06****
• Workers' Compensation (weeks)	-0.21	-0.13	-0.23	-0.16
• Training allowances per year (dollars)	-345.23	-54.30	-364.07	-381.67
H. Criminality				
• Total number of civilian arrests per year	-0.060***	-0.055***	-0.015	0.008
• Number of civilian theft arrests per year	-0.056***	-0.056***	-0.036*	-0.066***
• Probability in jail during interview week	-0.0003	N.A.	0.019	N.A.

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).

**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).

***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).

****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] The estimates given here are the same as in Table III.7, except for being placed on an annualized basis as noted. Most of the variables are estimated for the civilian population only (for more on the variable definitions and computations, see notes for Tables III.2 to III.6).

TABLE III.9

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS OF JOB CORPS ON A PERCENTAGE BASIS[a]

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
A. Employment and earnings of civilians				
• Employed (weeks)	3	12*	17***	20****
• Hours worked per week	10	13*	17***	21***
• Earnings per year (dollars)	5	10	13*	12*
• Employed, in school, or in training (weeks)	-8	4	6	9*
• Employed or looking for work (weeks)	4	5	7*	7*
• Employed in union job (weeks)	-35	-21	-19	-12
• Employed in PSE job (weeks)	-32	-4	19	17
B. Probability in military during interview week	58***	N.A.	77***	N.A.
C. Education and training of civilians				
• Probability of having high school diploma or GED by time of interview	34****	N.A.	230****	N.A.
• In any school (weeks)	-46****	-16	-32*	-20
• In college (weeks)	1,400*	2,200***	2,500***	1,797****
• In high school (weeks)	-71****	-61****	-79****	-88****
• In vocational or technical school (weeks)	350	1,357***	265*	272*
• In other school (weeks)	-52***	-42*	-57**	-41**
• In any training program (weeks)	-28	9	-28	-28
• In CETA training (weeks)	-55**	-36*	-57***	-43*
• In WIN training (weeks)	-91*	-96***	-94****	-85***
• In other training (weeks)	30	1,278*	800*	400
D. Number of moves for civilians (mobility)				
• All moves across cities (cumulative)	307****	N.A.	113***	N.A.
• For job opportunities (cumulative)	767****	N.A.	65*	N.A.
• For education or training (cumulative)	492****	N.A.	45	N.A.
• All moves across cities exculding Job Corps relocations (cumulative)	214****	N.A.	111***	N.A.

In Chapter V we will compare the dollar value of these benefits to the dollar value of program cost in order to estimate whether the program represents an efficient use of resources. First, however, we turn to an exploratory analysis both of the effects of Job Corps on family composition and of the employability and related impacts for the small subgroup of Corpswomen who have children living with them during the postprogram observation period.

APPENDIX TO CHAPTER III

ESTIMATION PROCEDURES AND REPRESENTATIVE ESTIMATES

This appendix is designed to document the estimation procedures underlying the findings presented in the text of Chapter III, and to provide the details of some representative estimates. By necessity, this appendix is more technical than the rest of the report, and readers with a non-technical interest will probably want only to skim this appendix or to proceed directly to Chapter IV.

Our basic econometric model of behavior is a single-equation regression that can be represented as:

$$Y_{it} = \underset{\sim}{\beta}'\underset{\sim}{X}_{it} + \underset{\sim}{T}\underset{\sim}{T}_{it} + \varepsilon_{it}, \qquad (1)$$

where Y_{it} is the economic behavior of interest (e.g., employment and related activities) for the i^{th} individual during the t^{th} time period; the β's are coefficients; the X's are exogenous variables and lagged values of dependent variables from pre-enrollment that explain the behavior of interest; the T's are program effects on the behavior of interest; the T's are program variables; and ε is an error term.[1]

With a nonrandomized control group (a comparison group) the T's are potentially correlated with ε. Any unobserved variable that affects the economic behavior of interest is also likely to affect individual decisions about whether to participate in the program and, hence, the T's. Therefore, the T's are potentially endogenous with respect to the behavior of interest, in which case ordinary least squares (OLS) estimators will generally be biased.

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[1] The types of behaviors of interest in this study are related. Therefore, some gains in statistical efficiency could be achieved by using a multiple-equation technique that accounts for correlations in error terms across equations, such as a seemingly unrelated regression procedure. However, the gains in statistical efficiency with a seemingly unrelated regression approach would be small at best, because the explanatory variables are nearly identical for all of the types of behavior that we examine.

As an example, variables such as motivation and innate ability are not observed directly; undoubtedly, they affect both employment-related behavior and the decision whether to participate in Job Corps or in other similar programs. Thus, these unobserved variables can be important elements in the error terms for both Y and the T's with a comparison group (with a randomized control group unobserved as well as observed, variables should be orthogonal or uncorrelated with the T's). Therefore, the T's will generally be correlated with ε when a comparison group is used, and the program variables should be treated as endogenous. Furthermore (as is well known), OLS estimators will generally be biased when endogenous variables are included in regression equations, unless the correlation between the endogenous variables and the error term (ε) can be netted out.

Different procedures for selecting comparison groups in the absence of randomization will result in varying levels of (1) statistical inefficiency from correlation between the X's and the T's, and (2) OLS bias from correlation between the T's and ε when the program effects, T's, are estimated via OLS. Some procedures that have been used to obtain comparison samples yield very poor matches and, hence, are very inaccurate estimates for the T's from either a difference-in-sample-means or OLS-estimation procedure (e.g., evaluations that rely on the use of before-after comparisons with youths or other re-entrants or new entrants to the labor force, and on the use of individuals who enrolled but did not show up for the program or who dropped out of the program very soon after entering).

TABLE III.8

SUMMARY OF MAIN FINDINGS FOR OVERALL IMPACTS OF JOB CORPS ON AN ANNUALIZED BASIS[a]

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
A. Employment and earnings of civilians				
• Employed (weeks)	0.62	2.63*	4.05***	4.70****
• Hours worked per week	1.33	1.98*	3.17***	3.66***
• Earnings per year (dollars)	105.17	312.16	503.88*	469.33*
• Employed, in school, or in training (weeks)	-2.04	1.22	1.70	2.66*
• Employed or looking for work (weeks)	1.51	1.75	2.39*	2.51*
• Employed in union job (weeks)	-1.12	-0.76	-0.83	-0.50
• Employed in PSE job (weeks)	-0.72	-0.09	0.50	0.34
B. Probability in military during interview week	0.019***	N.A.	0.039***	N.A.
C. Education and training of civilians				
• Probability of having high school diploma or GED by time of interview	0.052****	N.A.	0.250****	N.A.
• In any school (weeks)	-2.80****	-0.88	-1.54*	-0.99
• In college (weeks)	0.75*	1.13***	1.29***	1.54****
• In high school (weeks)	-3.05****	-2.32****	-2.56****	-2.56****
• In vocational or technical school (weeks)	0.48	1.00***	0.64*	0.75*
• In other school (weeks)	-1.03***	-0.71*	-0.90**	-0.74**
• In any training program (weeks)	-0.50	0.17	-0.51	-0.53
• In CETA training (weeks)	-0.79**	-0.57*	-0.95***	-0.70*
• In WIN training (weeks)	-0.11*	-0.17***	-0.21****	-0.21***
• In other training (weeks)	0.07	0.43*	0.43*	0.19
D. Number of moves for civilians (mobility)				
• All moves across cities (cumulative)	0.273****	N.A.	0.270***	N.A.
• For job opportunities (cumulative)	0.184****	N.A.	0.149*	N.A.
• For education or training (cumulative)	0.059****	N.A.	0.030	N.A.
• All moves across cities exculding Job Corps relocations (cumulative)	0.174****	N.A.	0.215***	N.A.

Table III.9 (continued)

Variable	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
E. Serious health problems for civilians (weeks)	-19	-12	-20	-27
F. Receipt of public assistance by civilians				
• Any financial assistance (weeks)	-57****	-49****	-44****	-52****
• AFDC (weeks)	-47****	-39****	-39****	-46****
• General Assistance or other (weeks)	-75****	-74****	-62****	-61****
• Food Stamps (weeks)	-11	-9	-4	-1
• Public housing (weeks)	10	-8	-5	-18
G. Receipt of other transfers by civilians				
• Unemployment Insurance (weeks)	-87****	-81****	-63****	-50****
• Workers' Compensation (weeks)	-85	-83	-67	-49
• Training allowances per year (dollars)	-27	-7	-64	-69
H. Criminality				
• Total number of civilian arrests per year	-32***	-37***	-10	5
• Number of civilian theft arrests per year	-50***	-59***	-39*	-64***
• Probability in jail during interview week	-1	N.A.	167	N.A.

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).

**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).

***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).

****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

N.A. - Not Applicable

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a/] The estimates presented in this table are the same as in Table III.7, except that they are made on a percentage basis, with the estimates given in Table III.1 as the base. Most of the variables are estimated for the civilian population only (for more on the variable definitions and computations, see notes for Tables III.2 to III.6).

Our comparison-group, survey, and econometric procedures were designed to yield as efficient estimates as possible.[1] The basic comparison-group procedure consisted of randomly selecting eligible non-participants who were similar to Corpsmembers in observable characteristics and who lived in geographic areas similar to the neighborhoods where Corpsmembers lived before entering Job Corps centers. (Sites that were very proximate to centers--neighborhoods within the three-digit zip-code areas where centers were located--were eliminated from being chosen as comparison sites because knowledge of the Job Corps program was likely to be prevalent in these sites.)

The comparison-group procedure was designed to yield a sample of youths who were were similar to Corpsmembers but who did not go into the program, largely because they had little (or no) knowledge about the program. This comparison strategy was feasible for three reasons: (1) there are large numbers of eligible youths who are similar to participating Corpsmembers who do not attempt to enter Job Corps, (2) program participation is geographically clustered because of limited recruitment, and (3) most Corpsmembers first learn about the program from friends or relatives (see Kerachsky and Mallar, 1978).

Our comparison-group procedure should yield a sample of youths who are similar to Corpsmembers but who do not participate largely because they do not know about the program. Therefore, it yields a very efficient comparison group, and estimates of program effects should not be sensitive to varying assumptions about observable and unobservable differences between the program and comparison groups as compared to other potential comparison groups (see Mallar, 1979). Because comparison-group members differ from Corpsmembers primarily in terms of random access to information about the program and random proximity to the program, T and ε at best should be weakly correlated.[1]

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[1] For more details, see Mallar (1979), as well as Technical Reports A, B, C, H, and J.

Despite the use of the rigorous comparison-group procedures outlined above, however, the interview data should be used to control for observed and unobserved differences that remain between the comparison and participant groups in order to ensure against bias in the estimates of program effects. Consistent estimates can be obtained by controlling for observed differences directly and by controlling for unobserved differences indirectly by modeling the participation decision.[2]

If a normal distribution is assumed for the error term in equation (1), this error term can then be decomposed into an unobserved, explanatory variable plus a new error term that is uncorrelated with the T's, as follows:

$$Y_{it} = \underset{\sim}{\beta}'\underset{\sim}{X}_{it} + \underset{\sim}{\pi}\underset{\sim}{T}_{it} + \delta\lambda_i + \omega_{it}, \tag{2}$$

where

$$\lambda_i = P_i \frac{f(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)}{F(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)} - (1 - P_i) \frac{f(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)}{1 - F(\underset{\sim}{\alpha}'\underset{\sim}{Z}_i)}, \tag{3}$$

P_i is a binary program participation variable that equals one for Corpsmembers (zero otherwise); $f(\cdot)$ denotes the standard normal density function for program participation; $F(\cdot)$ denotes the standard normal distribution function for program participation; the Z's are explanatory variables that affect program participation; and δ and the α's are coefficients.

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[1] Some of the discussion is phrased as if there were only one (binary) program variable, which may or may not be the case. However, the results generalize to multiple T's, as is the case for our application (see further below).

[2] For more details, see Heckman (1979), Barnow, Cain, and Goldberger (1978), and Mallar (1979).

The sign of δ is determined by the correlation of the errors (e.g., from omitted variables) in the underlying equations for Y and P. Therefore, if the estimated value for δ is positive, it indicates that individuals who are likely to participate for unobserved reasons will have higher values of Y, on average, for unobserved reasons, in which case the failure to adjust for sample selection bias will make program effects biased in a positive direction. Similarly, if the estimated value for δ is negative, it indicates that individuals who are likely to participate for unobserved reasons will have lower values of Y, on average, for unobserved reasons, in which case the failure to adjust for sample selection bias will make program effects biased in a negative direction.

As noted in the text, when estimating Job Corps effects on employment and related activities, the bias stemming from the failure to adjust for sample selection could be in either direction. A positive δ and positive bias will result if there is a predominance of youths with higher innate abilities and stronger motivation who are more likely to participate in Job Corps because they benefit more; a negative δ and negative bias will result if there is a predominance of youths with lower innate abilities who are more likely to participate in Job Corps because it costs them less (fewer opportunities outside of Job Corps).

If a consistent estimate of $F(\cdot)$ is obtained through probit procedures, then consistent estimates for equation (2) can be obtained by substituting the resulting predicted values of λ_i into equation (2). One issue that arises with these procedures for controlling for unobserved differences between program and comparison groups is how to identify equations (1) and (2) when predicted values of λ are used. Conceptually, as suggested by Barnow, Cain, and Goldberger (1978), equations (1) and (2) are identified by the inherent nonlinearities in λ, even if the X's and Z's are identical. In practice, however, these nonlinearities often turn out to be ineffective for identifying behavioral models, as in equations (1) and (2), and parameter restrictions are needed for identification. The parameter restrictions amount to observing variables that can be reasonably modeled as affecting the decision to participate in the program but not directly affecting the behavior of interest.

In our case, we have variables that relate to participation in Job Corps from the pre-enrollment neighborhoods of sample members (both Job Corps and comparison) for years prior to the survey (specifically, fiscal year 1975). These variables affect the knowledge of the program and, hence, the decision to participate. However, they can reasonably be assumed not to affect the behavior of interest, which is supported by comparing the pre-enrollment neighborhoods where our Corpsmembers and comparison sample lived. The Job Corps and comparison neighborhoods were very similar in terms of employment, income, racial/ethnic, and age distributions that are relevant for the labor-market behavior of youths (see Kerachsky and Mallar, 1978).

Once the predicted λ variable was added, the correlation of error terms for the same individual over time was accounted for in an error-components (or variance-components) model. This model should yield greater efficiency for coefficient estimates and more accurate estimates of standard errors than OLS (for more details, see Maddala, 1971; Nerlove, 1971a and

1971b; and Wallace and Hussain, 1969).[1] A two-stage procedure was used with the variance component for individual youths, estimated from a first-stage OLS regression (including the predicted λ variable) and then substituted into a second-stage generalized least squares framework.[2] The computational program enabled us to include individuals with varying lengths of time, and allowed individuals to be missing periods of data (early, late, or intervening quarters), which is essential for our application because Corpsmembers' postprogram data start at varying points in time.[3] Seasonality and time trends across individuals are specified explicitly in the regression equations (there is only an individual component, and no time component, included in the error term for our econometric model).

The explanatory and predetermined variables used as controls in our regressions are documented in Table III.10. These variables control for age (5 variables); pre-enrollment education (3 variables); race/ethnicity (4 variables); pre-enrollment health (1 variable); seasonality

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[1] The point-in-time probability models with binary dependent variables (e.g., the probabilities of military service during the survey week, of having a high school diploma or GED by the survey week, and of being in jail during the survey week) were estimated with probit maximum likelihood techniques, with one observation per individual youth.

[2] The lagged values of dependent variables from pre-enrollment cannot reasonably be assumed to be strictly predetermined when we pool observations for individual youths over time. These lagged dependent variables are endogenous in general if we assume (as we must) that equation error terms are correlated over time for individuals. However, the use of estimators of error variances and covariances from ordinary least squares residuals will still yield consistent estimators of coefficients in a second-stage generalized least squares technique when the error-components model is appropriate, as we assume in our estimation procedure (for more details and proofs, see Wallace and Hussain, 1969, and Nerlove, 1971a and 1971b).

[3] For documentation of the computer program, see Avery (1975).

(3 variables) and time trends (2 variables); and pre-enrollment experiences with employment (1 variable), welfare (1 variable), illegal activities (1 variable), and drug use (2 variables). The variables used in the Job Corps participation, employment, and related equations are similar except for the two variables on youths from pre-enrollment neighborhoods who enrolled in Job Corps during fiscal year 1975, which are included in the Job Corps participation equation but not in the equations for employment and related behavior.

The probit estimates for the probability of being in Job Corps are shown in Tables III.11 and III.12 for males and females without children, respectively. The two most important points to note are that (1) the identifying variables (#JCMEN-75 and %JCMEN-75) are highly significant and, hence, will provide good identification, and (2) these equations are exceptionally good predictors of who is in the Job Corps versus comparison groups, as evidenced by the exceptionally high chi-square statistic for the equation.

Tables III.13 and III.14 show our regression estimates for employment, Tables III.15 and III.16 show our regression estimates for college attendance, and Tables III.17 and III.18 show our regression estimates for number of arrests.[1/] The coefficients on the Job Corps variables in these

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[1/] The standard errors and t-statistics given in Tables III.13 through III.18 may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics shown are approximately accurate and are indicative of the true values of these statistics. Maximum likelihood estimates could yield some gains in terms of the statistical efficiency for coefficient estimates and of unbiasedness for estimates of the standard errors. With our large sample sizes, however, maximum likelihood estimation would be prohibitively expensive.

representative regression equations are the ones used to construct the tables in the text. For employment and college attendance, the coefficient estimates for the lambda variables (i.e., δ) are small, negative, and statistically insignificant. This indicates that Corpsmembers would tend to have slightly lower employability and college attendance than the comparison sample in the absence of Job Corps, and that slightly smaller estimates of the positive Job Corps effects would be obtained if we did not control for unobserved differences between the Corpsmember and the comparison groups. The coefficient estimates for the lambda variables (i.e., δ) in the arrest equations are small, positive, and statistically insignificant. This indicates that Corpsmembers would tend to be slightly more criminally inclined than the comparison sample in the absence of Job Corps, and that slightly smaller estimates of reductions in crime for Corpsmembers would be obtained if we did not adjust for unobserved differences between the Job Corps and comparison groups.

There are also interesting aspects to the other control variables, which will not be fully developed here because they are not of primary interest. Some of these other effects can be highlighted briefly as follows: (1) youths generally do better in the labor market as they become older; (2) youths with higher pre-enrollment educations generally do better both in the labor market and at college attendance; (3) minority youths generally do worse than whites in the labor market; (4) employment is highest in the summer and fall, and college attendance is lowest in the summer for youths; (5) the employability of youths was improving over the short postprogram time period but at a slower rate as time passed; and (6) youths who had better pre-enrollment work and related histories generally did much better than other youths in those activities during subsequent time periods.

CHAPTER III

APPENDED TABLES

TABLE III.10

GLOSSARY OF EXPLANATORY VARIABLES[a]

Variable Label[b]	Definition[c]
AGE =	the youth's age in years.
AGE18 =	1 if the youth is at least eighteen years old; 0 otherwise (allows for an intercept change at eighteen).
OVER18 =	AGE minus 18 if AGE if greater than eighteen; 0 otherwise (allows for a slow change at eighteen).
AGE21 =	1 if the youth is at least twenty-one years old; 0 otherwise (allows for an intercept change at twenty-one).
OVER21 =	AGE minus 21 if AGE is greater than twenty-one; 0 otherwise (allows for a slope change at twenty-one).
EDUCATION-PRE =	the youth's highest grade of formal education in years completed before the Job Corps enrollment date (preenrollment--defined as six months before the baseline interview for the comparison sample).
DIPLOMA-PRE =	1 if the youth had a high school diploma or equivalency (allows for an intercept change with high school diploma or equivalency).
EDUCATION12-PRE =	1 if the youth had completed at least twelve years of formal education at preenrollment (allows for an additional intercept change at high school diploma).
EDOVER12-PRE =	EDUCATION-PRE minus 12 if EDUCATION-PRE is greater than 12; 0 otherwise (allows for an intercept change at twelve).
COLLEGE-PRE =	1 if the youth ever attended college before enrollment; 0 otherwise.
BLACK =	1 if the youth is black and not of Hispanic origin; 0 otherwise.
HISPANIC =	1 if the youth is a person of Mexican, Puerto Rican, Cuban, Central or South American, or other Spanish culture or origin, regardless of race; 0 otherwise.
AMERICAN INDIAN =	1 if the youth is an American Indian or Alaskan native; 0 otherwise.
OTHER RACE/ETH =	1 if the youth is from a race/ethnicity other than WHITE, BLACK, HISPANIC, or AMERICAN INDIAN (mostly Asian or Pacific Islander); 0 otherwise.
HEALTH PROB-BASE =	1 if the youth reported a serious health problem in the baseline interview that both limited the kind or amount of work that (s)he could do and had lasted for at least one year (misses health problems present at preenrollment that were cured before the baseline interview); 0 otherwise.
FALL =	1 if the quarter is during the fall season (September, October, and November); 0 otherwise.

Table III.10 (continued)

Variable[b]	Definition[c]
CAT1 12 to 18 =	1 if the youth is a program completer and the quarter is twelve to eighteen months after (s)he terminated from Job Corps; 0 otherwise.
CAT1 18 to 24 =	1 if the youth is a program completer and the quarter is eighteen to twenty-four months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 0 to 6 =	1 if the youth is a partial completer and the quarter is zero to six months after (s)he terminated from Job Corps; 0 otherwise
CAT2 6 to 12 =	1 if the youth is a partial completer and the quarter is six to twelve months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 12 to 18 =	1 if the youth is a partial completer and the quarter is twelve to eighteen months after (s)he terminated from Job Corps; 0 otherwise.
CAT2 18 to 24 =	1 if the youth is a partial completer and the quarter is eighteen to twenty-four months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 0 to 6 =	1 if the youth is an early dropout and the quarter is zero to six months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 6 to 12 =	1 if the youth is an early dropout and the quarter is six to twelve months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 12 to 18 =	1 if the youth is an early dropout and the quarter is twelve to eighteen months after (s)he terminated from Job Corps; 0 otherwise.
CAT3 18 to 24 =	1 if the youth is an early dropout and the quarter is eighteen to twenty-four months after (s)he terminated from Job Corps; 0 otherwise.

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[a] The lagged values of dependent variables from preenrollment cannot reasonably be assumed to be strictly predetermined when we pool observations for individual youths over time. These lagged dependent variable are endogenous in general if we assume (as we must) that equation error terms are correlated over time for individuals. However, the use of estimators of error variances and covariances from ordinary least squares residuals will still yield consistent estimators of coefficients in a second-stage generalized least squares technique when the error-components model is appropriate, as we assume in our estimation procedure (for more details and proofs, see Wallace and Hussain, 1969, and Nerlove, 1971a and 1971b).

[b] The explanatory variables are arranged in this table approximately in the order that findings are presented in subsequent tables.

[c] The preenrollment period is defined as six months before the baseline interview for the comparison sample, because Corpsmembers had been in the program approximately six months on average when the comparison youths were interviewed. The data are arrayed into quarterly aggregates by calendar quarters according to the seasons--summer (June, July, and August), fall (September, October, and November), winter (December, January, and February), and spring (March, April, and May)--which differ from the usual fiscal quarters but provide better controls for seasonality. Time-dependent variables--such as those related to age, calendar time, and length of time out of Job Corps--are defined for the midpoint of each quarter.

TABLE III.II

PROBIT ESTIMATES FOR THE PROBABILITY OF BEING IN THE JOB CORPS SAMPLE: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[c]	Partial Derivative at Point of Means[d]
CONSTANT	-2.297	1.047	-2.193	-0.375
AGE	0.214	0.064	3.329	0.035
AGE18	-0.006	0.083	-0.070	-0.001
OVER18	-0.521	0.117	-4.464	-0.085
AGE21	-0.681	0.281	-2.425	-0.111
OVER21	-0.344	0.171	-0.201	-0.056
EDUCATION-PRE	-0.138	0.025	-5.420	-0.022
DIPLOMA-PRE	-0.278	0.144	-1.932	-0.045
EDUCATION12-PRE	2.588	4.623	0.560	0.423
EDOVER12-PRE	1.021	0.172	5.945	0.167
BLACK	-0.014	0.061	-0.225	-0.002
HISPANIC	-0.067	0.097	-0.689	-0.011
AMERICAN INDIAN	0.342	0.147	2.319	0.056
HEALTHPROB-BASE	0.133	0.130	1.020	0.022
ANYEMPLOY-PRE	0.137	0.055	2.507	0.022
ANYSELF-PRE	0.060	0.104	0.575	0.010
ANYARRESTS-PRE	0.180	0.069	2.593	0.029
MJ/ALCOHOL-PRE	0.055	0.064	0.872	0.009
COKE/HEROIN-PRE	0.516	0.100	5.149	0.084
#JCMEM-75	0.002	0.0004	3.989	0.0003
%JCMEM-75	0.006	0.0004	13.663	0.001

Number of observations = 4,155

Mean of dependent variable = 0.829

Chi-Square statistic for equation = 698.134

- Degrees of freedom = 20
- Significance level = > 99% statistical confidence

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[a] Maximum likelihood estimates are computed by an iterative Newton-Rapheson procedure.

[b] For definitions of explanatory variables, see Table III.10.

[c] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

[d] The change in probability associated with a marginal change in the relevant explanatory variable equals the coefficient estimate times the value of the density function which is obtained here with the mean values for all explanatory variables (i.e., the point of means). For binary explanatory variables it is more appropriate to use the difference between the distribution function values with and without the relevant coefficient with all other explanatory variables at their mean values. This latter approach is used in this report to obtain impact estimates for probability variables.

TABLE III.12

PROBIT ESTIMATES FOR THE PROBABILITY OF BEING IN THE JOB CORPS SAMPLE: FEMALES WITHOUT CHILDREN[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic	Partial Derivative at Point of means
CONSTANT	-3.525	1.312	-2.687	-0.624
AGE	0.191	0.080	2.386	0.034
AGE18	-0.204	0.111	-1.838	-0.036
OVER18	-0.153	0.156	-0.983	-0.027
AGE21	1.641	6.995	0.235	0.291
OVER21	0.302	0.280	1.079	0.054
EDUCATION-PRE	-0.085	0.043	-1.969	-0.015
DIPLOMA-PRE	-0.065	0.229	-0.285	-0.012
EDUCATION12-PRE	0.021	0.353	0.060	0.004
EDOVER12-PRE	1.064	0.253	4.207	0.188
BLACK	0.592	0.092	6.419	0.105
HISPANIC	0.719	0.148	4.857	0.127
AMERICAN INDIAN	1.151	0.243	4.738	0.204
HEALTHPROB-BASE	0.060	0.193	0.313	0.011
ANYEMPLOY-PRE	0.235	0.084	2.809	0.042
ANYWELF-PRE	-0.223	0.149	-1.494	-0.040
ANYARRESTS-PRE	-0.085	0.198	-0.430	-0.015
MJ/ALCOHOL-PRE	0.218	0.087	2.512	0.039
COKE/HEROIN-PRE	0.381	0.156	2.442	0.067
#JCMEM-75	0.003	0.001	4.498	0.001
%JCMEM-75	0.007	0.001	8.886	0.001

Number of observations = 1,710

Mean of dependent variable = 0.760

Chi-Square statistic for equation = 555.672

- Degrees of freedom = 20
- Significance level = > 99% statistical confidence

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[a] Maximum likelihood estimates are computed by an iterative Newton-Rapheson procedure.

[b] For definitions of explanatory variables, see Table III.10.

[c] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

[d] The change in probability associated with a marginal change in the relevant explantory variable equals the coefficient estimate times the value of the density function which is obtained here with the mean values for all explanatory variables (i.e., the point of means). For binary explanatory variables it is more appropriate to use the difference between the distribution function values with and without the relevant coefficient with all other explanatory variables at their mean values. This latter approach is used in this report to obtain impact estimates for probability variables.

TABLE III.13

REGRESSION ESTIMATES FOR FRACTION OF TIME EMPLOYED: MALES[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error	T-Statistic[d/]
CONSTANT	0.061	0.393	0.154
AGE	0.008	0.023	0.327
AGE18	0.055	0.027	2.025
OVER18	-0.006	0.024	-0.270
AGE21	-0.007	0.014	-0.498
OVER21	0.018	0.012	1.466
EDUCATION-PRE	0.006	0.006	0.963
DIPLOMA-PRE	0.088	0.036	2.418
EDUCATION12-PRE	-0.054	0.043	-1.262
BLACK	-0.124	0.015	-8.201
HISPANIC	-0.058	0.022	-2.619
AMERICAN INDIAN	-0.168	0.035	-4.858
OTHER RACE/ETH	-0.098	0.057	-1.711
HEALTHPROB-BASE	-0.0005	0.029	-0.016
FALL	0.003	0.008	0.341
WINTER	-0.041	0.008	-4.877
SPRING	-0.036	0.009	-4.062
MONTHS	0.028	0.003	10.474
MONTHS SQUARED	-0.001	0.0001	-7.876
%EMPLOY-PRE	0.158	0.017	9.147
%WELF-PRE	-0.061	0.032	-1.889
ANYARRESTS-PRE	-0.020	0.016	-1.235
MJ/ALCOHOL-PRE	0.028	0.015	1.873
COKE/HEROIN-PRE	-0.009	0.022	-0.410
LAMBDA	-0.039	0.027	-1.472
CAT1 0 To 6	0.057	0.044	1.303
CAT1 6 To 12	0.131	0.044	2.964
CAT1 12 To 18	0.155	0.044	3.490
CAT1 18 To 24	0.194	0.047	4.162
CAT2 0 To 6	-0.028	0.045	-0.614
CAT2 6 To 12	0.049	0.045	1.079
CAT2 12 To 18	0.044	0.046	0.960
CAT2 18 To 24	0.076	0.048	1.598
CAT3 0 To 6	-0.001	0.052	-0.024

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[d]
CAT3 6 To 12	-0.001	0.053	-0.015
CAT3 12 To 18	0.047	0.054	0.881
CAT3 18 To 24	0.083	0.054	1.538

Number of observations = 14,506

- Number of individuals = 2,336
- Average number of time periods = 6.209

Intraclass correlation coefficient (proportion of error variance attributable to individual component)=0.372

Mean of dependent variable = 0.546

F-Statistic for equation = 35.902

- Degrees of freedom = 36; 14,469
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.10.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

TABLE III.14

REGRESSION ESTIMATES FOR FRACTION OF TIME EMPLOYED: FEMALES WITHOUT CHILDREN[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	-0.148	0.546	-0.271
AGE	0.009	0.032	0.287
AGE18	-0.011	0.040	-0.271
OVER18	0.003	0.034	0.077
AGE21	0.005	0.021	0.244
OVER21	0.027	0.019	1.423
EDUCATION-PRE	0.022	0.010	2.201
DIPLOMA-PRE	0.019	0.056	0.337
EDUCATION12-PRE	0.016	0.063	0.252
BLACK	-0.151	0.026	-5.690
HISPANIC	-0.027	0.039	-0.694
AMERICAN INDIAN	-0.151	0.068	-2.218
OTHER RACE/ETH	0.021	0.099	0.209
HEALTHPROB-BASE	-0.056	0.048	-1.178
FALL	0.054	0.012	4.504
WINTER	0.005	0.012	0.389
SPRING	-0.030	0.013	-2.322
MONTHS	0.008	0.004	2.096
MONTHS SQUARED	-0.0001	0.0001	-0.596
%EMPLOY-PRE	0.171	0.029	5.832
%WELF-PRE	-0.095	0.040	-2.356
ANYARRESTS-PRE	-0.111	0.048	-2.322
MJ/ALCOHOL-PRE	0.026	0.021	1.245
COKE/HEROIN-PRE	0.012	0.038	0.324
LAMBDA	-0.006	0.033	-0.190
CAT1 0 To 6	0.070	0.052	1.348
CAT1 6 To 12	0.172	0.052	3.289
CAT1 12 To 18	0.176	0.053	3.310
CAT1 18 To 24	0.220	0.058	3.826
CAT2 0 To 6	-0.004	0.055	-0.080
CAT2 6 To 12	0.059	0.055	1.058
CAT2 12 To 18	0.081	0.057	1.413
CAT2 18 To 24	0.058	0.063	0.913

Table III.14 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[d]
CAT3 0 To 6	0.018	0.072	0.245
CAT3 6 To 24	-0.030	0.076	-0.403
CAT3 12 To 18	0.054	0.079	0.690
CAT3 18 To 24	-0.095	0.083	-1.144

Number of observations = 5,887

- Number of individuals = 1,042
- Average number of time periods = 5.649

Intraclass correlation coefficient (proportion of error variance attributable to individual component)=0.423

Mean of dependent variable = 0.392

F-Statistic for equation = 14.903

- Degrees of freedom = 36; 5,850
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.10.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

TABLE III.15

REGRESSION ESTIMATES FOR FRACTION OF TIME IN COLLEGE: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	-0.015	0.092	-0.168
AGE	-0.001	0.005	-0.215
AGE18	0.003	0.006	0.493
OVER18	0.002	0.006	0.369
AGE21	-0.005	0.003	-1.391
OVER21	-0.003	0.003	-1.186
EDUCATION-PRE	0.002	0.002	1.325
DIPLOMA-PRE	-0.006	0.010	-0.592
EDUCATION12-PRE	0.022	0.012	1.902
BLACK	-0.0001	0.004	-0.026
HISPANIC	0.011	0.006	1.738
AMERICAN INDIAN	0.003	0.010	0.322
OTHER RACE/ETH	-0.0001	0.016	-0.007
HEALTHPROB-BASE	-0.009	0.008	-1.091
FALL	0.008	0.002	4.511
WINTER	0.011	0.002	5.569
SPRING	0.009	0.002	4.253
MONTHS	0.0002	0.001	0.296
MONTHS SQUARED	-0.00002	0.00002	-0.863
%EMPLOY-PRE	0.002	0.005	0.510
%WELF-PRE	-0.005	0.009	-0.609
ANYARRESTS-PRE	0.003	0.004	0.622
MJ/ALCOHOL-PRE	-0.003	0.004	-0.642
COKE/HEROIN-PRE	0.009	0.006	1.513
COLLEGE-PRE	0.017	0.026	0.652
LAMBDA	-0.010	0.007	-1.405
CAT1 0 To 6	0.019	0.012	1.538
CAT1 6 To 12	0.029	0.012	2.405
CAT1 12 To 18	0.035	0.012	2.876
CAT1 18 To 24	0.030	0.013	2.405
CAT2 0 To 6	0.014	0.012	1.113
CAT2 6 To 12	0.020	0.012	1.571
CAT2 12 To 18	0.023	0.013	1.873

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Table III.15 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[d]
CAT2 18 To 24	0.023	0.013	1.816
CAT3 0 To 6	0.014	0.014	0.999
CAT3 6 To 12	0.015	0.014	1.076
CAT3 12 To 18	0.019	0.015	1.335
CAT3 18 To 24	0.021	0.015	1.434

Number of observations = 14,506

- Number of individuals = 2,336
- Average number of time periods = 6.209

Intraclass correlation coefficient (proportion of error variance attributable to individual component)=0.492

Mean of dependent variable = 0.013

F-Statistic for equation = 3.222

- Degrees of freedom = 37; 14,468
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.10.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

TABLE III.16

REGRESSION ESTIMATES FOR FRACTION OF TIME IN COLLEGE: FEMALES WITHOUT CHILDREN[a/]

Explanatory Variable[b/]	Coefficient Estimate	Standard Error[c/]	T-Statistic[d/]
CONSTANT	-0.115	0.220	-0.522
AGE	0.001	0.013	0.097
AGE18	0.022	0.016	1.387
OVER18	-0.016	0.014	-1.173
AGE21	0.001	0.008	0.169
OVER21	0.004	0.008	0.552
EDUCATION-PRE	0.006	0.004	1.430
DIPLOMA-PRE	0.049	0.025	2.005
EDUCATION12-PRE	-0.004	0.028	-0.143
BLACK	-0.012	0.012	-1.062
HISPANIC	-0.001	0.017	-0.058
AMERICAN INDIAN	-0.034	0.030	-1.151
OTHER RACE/ETH	0.009	0.043	0.217
HEALTHPROB-BASE	0.021	0.021	0.984
FALL	0.028	0.005	5.813
WINTER	0.024	0.005	5.038
SPRING	0.017	0.005	3.248
MONTHS	-0.0003	0.002	-0.228
MONTHS SQUARED	0.0001	0.0001	0.966
%EMPLOY-PRE	0.031	0.013	2.429
%WELF-PRE	0.052	0.018	2.926
ANYARRESTS-PRE	0.002	0.021	0.100
MJ/ALCOHOL-PRE	0.003	0.009	0.361
COKE/HEROIN-PRE	-0.013	0.017	-0.808
COLLEGE-PRE	0.052	0.039	1.330
LAMBDA	-0.015	0.014	-1.050
CAT1 0 To 6	0.029	0.023	1.282
CAT1 6 To 12	0.047	0.023	2.054
CAT1 12 To 18	0.043	0.023	1.858
CAT1 18 To 24	0.060	0.025	2.424
CAT2 0 To 6	0.015	0.024	0.636
CAT2 6 To 12	0.024	0.024	1.010
CAT2 12 To 18	0.023	0.025	0.928

Table III.16 (continued)

Explantory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CAT2 18 To 24	0.025	0.027	0.928
CAT3 0 To 6	0.007	0.031	0.217
CAT3 6 To 12	0.025	0.033	0.768
CAT3 12 To 18	0.027	0.034	0.793
CAT3 18 To 24	0.071	0.035	2.015

Number of observations = 5,887

- Number of individuals = 1,042
- Average number of time periods = 5.649

Intraclass correlation coefficient (proportion of error variance attributable to individual component)=0.490

Mean of dependent variable = 0.037

F-Statistic for equation = 4.712

- Degrees of freedom = 37; 5,849
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below).

[b] For definitions of explanatory variables, see Table III.10.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

TABLE III.17

REGRESSION ESTIMATES FOR NUMBER OF ARRESTS PER QUARTER: MALES[a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	0.100	0.247	0.407
AGE	-0.001	0.015	-0.042
AGE18	-0.008	0.018	-0.438
OVER18	-0.006	0.015	-0.424
AGE21	-0.002	0.009	-0.207
OVER21	0.010	0.006	1.519
EDUCATION-PRE	-0.002	0.002	-1.168
DIPLOMA-PRE	0.016	0.013	1.239
EDUCATION12-PRE	-0.011	0.015	-0.714
BLACK	-0.012	0.005	-2.285
HISPANIC	-0.008	0.008	-1.013
AMERICAN INDIAN	0.015	0.012	1.196
OTHER RACE/ETH	-0.004	0.020	-0.203
HEALTHPROB-BASE	-0.012	0.010	-1.167
FALL	0.001	0.006	0.212
WINTER	0.003	0.006	0.551
SPRING	-0.005	0.007	-0.721
MONTHS	-0.003	0.002	-1.726
MONTHS SQUARED	0.0001	0.0001	1.822
%EMPLOY-PRE	0.007	0.006	1.219
%WELF-PRE	-0.002	0.012	-0.133
ANYARRESTS-PRE	0.034	0.006	6.096
MJ/ALCOHOL-PRE	0.016	0.005	2.933
COKE/HEROIN-PRE	0.011	0.008	1.462
LAMBDA	0.010	0.009	1.073
CAT1 0 To 6	-0.026	0.016	-1.624
CAT1 6 To 12	-0.015	0.016	-0.914
CAT1 12 To 18	-0.001	0.017	-0.036
CAT1 18 To 24	-0.009	0.019	-0.485
CAT2 0 To 6	-0.004	0.017	-0.236
CAT2 6 To 12	-0.003	0.017	-0.189
CAT2 12 To 18	0.007	0.017	0.410
CAT2 18 To 24	0.032	0.020	1.612
CAT3 0 To 6	-0.023	0.021	-1.075

Table III.17 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[d]
CAT3 6 To 12	-0.027	0.022	-1.241
CAT3 11 To 18	-0.010	0.023	-0.455
CAT3 18 To 24	-0.016	0.023	-0.700

Number of observations = 14,506

- Number of individuals = 2,336
- Average number of time periods = 6.209

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.018

Mean of dependent variable = 0.040

F-Statistic for equation = 4.070

- Degrees of freedom = 36; 14,469
- Significance level = > 99% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below). The impact estimates in Tables III.6 and III.7 are twice as large as shown here because the numbers are presented on a six-month rather than quarterly basis in those tables.

[b] For definitions of explanatory variables, see Table III.10.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true value of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

TABLE III.18

REGRESSION ESTIMATES FOR NUMBER OF ARRESTS PER QUARTER: FEMALES WITHOUT CHILDREN [a]

Explanatory Variable[b]	Coefficient Estimate	Standard Error[c]	T-Statistic[d]
CONSTANT	0.063	0.094	0.673
AGE	-0.002	0.006	-0.438
AGE18	0.008	0.007	1.027
OVER18	0.004	0.006	0.694
AGE21	-0.007	0.004	-1.921
OVER21	-0.002	0.003	-0.776
EDUCATION-PRE	-0.002	0.001	-2.091
DIPLOMA-PRE	-0.004	0.005	-0.753
EDUCATION12-PRE	0.008	0.006	1.318
BLACK	-0.001	0.003	-0.474
HISPANIC	-0.003	0.004	-0.738
AMERICAN	0.004	0.007	0.580
OTHER RACE/ETH	-0.006	0.009	-0.639
HEALTHPROB-BASE	0.004	0.004	0.905
FALL	0.003	0.003	1.312
WINTER	-0.001	0.003	-0.197
SPRING	-0.003	0.003	-0.989
MONTHS	0.0005	0.001	0.618
MONTHS SQUARED	-0.00002	0.00003	-0.607
%EMPLOY-PRE	0.001	0.003	0.348
%WELF-PRE	-0.004	0.004	-0.973
ANYARRESTS-PRE	-0.002	0.005	-0.494
MJ/ALCOHOL-PRE	0.004	0.002	1.920
COKE/HEROIN-PRE	-0.001	0.004	-0.278
LAMBDA	0.005	0.003	1.634
CAT1 0 To 6	-0.007	0.005	-1.362
CAT1 6 To 12	-0.010	0.006	-1.850
CAT1 12 To 18	-0.011	0.006	-1.939
CAT1 18 To 24	-0.008	0.007	-1.094
CAT2 0 To 6	-0.006	0.006	-1.099
CAT2 6 To 12	-0.010	0.006	-1.543
CAT2 12 To 18	-0.002	0.007	-0.283
CAT2 18 To 24	0.008	0.008	0.946

Table III.18 (continued)

Explanatory Variable[b]	Coefficient Estimate	Standard Error	T-Statistic[d]
CAT3 0 To 6	-0.011	0.009	-1.320
CAT3 6 To 12	-0.011	0.010	-1.114
CAT3 12 To 18	-0.014	0.011	-1.319
CAT3 18 To 24	0.019	0.012	1.644

Number of observations = 5,887

- Number of individuals = 1,042
- Average number of time periods = 5.649

Intraclass correlation coefficient (proportion of error variance attributable to individual component) = 0.0

Mean of dependent variable = 0.005

F-Statistic for equation = 1.374

- Degrees of freedom = 36; 5,850
- Significance level = > 90% statistical confidence

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[a] Consistent generalized least squares estimates for civilians are obtained with a two-stage procedure under the assumptions of an error-components regression model (see Avery, 1975). A consistent estimate of LAMBDA is used based on the separate probability model of being in the Job Corps sample discussed previously. Using a consistent estimate of LAMBDA will not affect the consistency of coefficient estimates but may bias the standard errors and t-statistics (see footnote c below). The impact estimates in Tables III.6 and III.7 are twice as large as shown here because the numbers are presented on a six-month rather than quarterly basis in those tables.

[b] For definitions for explanatory variables, see Table III.10.

[c] The standard errors and t-statistics given in this table may be slightly biased because the estimates of the standard errors were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the standard errors and t-statistics from the regression program are usually very close to their unbiased counterparts, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case with our estimates). Therefore, the standard errors and t-statistics presented here are approximately accurate and are indicative of the true values of these statistics.

[d] The t-statistic equals the coefficient estimate divided by its standard error. However, the numbers in this column are more accurate than can be obtained from the preceding two columns because of less rounding error.

IV. CHANGES IN FAMILY COMPOSITION AND EMPLOYMENT-RELATED IMPACTS FOR FEMALES WITH CHILDREN

As our sample ages, two issues will become increasingly important: the effect of Job Corps on family composition, and its effect on the employment and related activities of females with children. In this chapter, the data from our interviews are used to investigate the empirical evidence on these issues, not with the intention of providing definitive findings, but in order to explore the relevance of future research in this area

Program effects on family composition are interesting not only in their own right, but also because there is substantial evidence that family composition affects the employment and related behavior of females.[1] Estimates based on our comparison-group data show significant behavioral differences in the labor-supply behavior of female youths on the basis of whether or not they have children living with them. (Such behavioral differences were small in magnitude and not statistically significant for male youths in the comparison group.)

The previous theoretical and empirical research in this area finds that the presence of children in general, and early child-bearing in particular, reduces the labor supply and earnings of women. Therefore, Job Corps effects on family composition can have important indirect effects on the employment and related activities of females, and we do not want to completely ignore these substantial impacts by focusing only on females without children. However, these effects are difficult to estimate given the apparent underlying differences between our sample of Corpswomen and females in the comparison group in terms of family-composition behavior.[1] For example, prior to enrolling in Job Corps, females in our Corpsmember sample were much less likely to be married or to have children, compared to the women in the comparison sample (see Kerachsky and Mallar, 1977).

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[1] For example, see Bowen and Finegan (1969), Preston (1972), and Trussell and Abowd (1979).

By the second follow-up interview, our sample of female Corpsmembers with children was still relatively small, comprising only approximately 7 percent of the Corpswomen on average during the follow-up period. In particular, we have only 215 postprogram observations for females with children, and the small number of observations this implies for some subgroups (e.g., early dropouts) adds greatly to the variability of estimates of Job Corps effects for this group. However, even though this group is too small to affect the overall short-term findings and although the estimates will be very imprecise, the long-run implications make it necessary to examine the findings on the employment and related activities of females with children.

A. INITIAL FINDINGS FOR EFFECTS ON FAMILY COMPOSITION

Little previous research has been conducted on the effects of employment and training programs on family composition. However, to the extent that these programs have an impact on employability, they can consequently be expected to have an affect on the incentives for family formation, family dissolution, and fertility. In addition, the residential setting, counseling, and ancillary services provided by Job Corps are also likely to affect the family-composition decisions of participating youths.

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[1] For further evidence on these sample differences, see the discussion below and Kerachsky and Mallar (1977).

Tables IV.1 and IV.2 summarize our empirical evidence on the impact of Job Corps on family composition. The estimates in these tables are based on probit probability models that control for the same independent variables as in Chapter III, with the addition of two variables that control for family status at pre-enrollment--whether living with parents at pre-enrollment, and whether a head of household at pre-enrollment. For females, we find reductions in family formation and fertility that are relatively large and generally significant. There are also reductions in family formation and fertility for male Job Corps participants; however, the effects for males are quite small in magnitude and are not statistically significant. Unfortunately, the female effects are somewhat clouded by the observed underlying differences in family composition between the Job Corps and comparison samples at pre-enrollment and baseline, which are substantial on observable criteria (see Kerachsky and Mallar, 1977) and are difficult to model adequately in a simple regression framework. More definitive findings would require a much more thorough modeling and research effort.

The implications of reductions in family formation and fertility for employment and related activities can be seen by comparing the bottom panel of Table IV.3 to the other two panels. Females without children generally have higher levels of employment and earnings, more education and training, less receipt of public assistance, and less criminal activity than do females with children. Thus, the empirical evidence that Job Corps participation leads to reductions in family formation and fertility suggests

TABLE IV.1

ESTIMATES OF JOB CORPS IMPACTS ON FAMILY COMPOSITION, BY SEX

Variable	Unweighted Postprogram Sample Mean[a]	Average Job Corps Effect for Program Completers	Average Job Corps Effect for Partial Completers	Average Job Corps Effect for Early Dropouts	Average Job Corps Effect for All Enrollees
A. MALES					
Probability . . .					
• married at second follow-up	0.116	0.007	-0.016	-0.036	-0.017
• married at second follow-up, given never married at pre-enrollment	0.101	-0.019	-0.042	-0.052	-0.039
• head of household at second follow-up	0.241	0.051	-0.016	-0.063	-0.015
• have children at second follow-up (including those living apart)	0.198	-0.019	-0.031	-0.026	-0.027
• have children living with them at second follow-up	0.080	0.014	-0.008	-0.021	-0.007
• have an extra-marital child by second follow-up	0.163	-0.029	-0.030	-0.003	-0.019
B. FEMALES					
Probability . . .					
• married at second follow-up	0.201	-0.079	-0.096*	-0.087	-0.087**
• married at second follow-up given never married at pre-enrollment	0.156	-0.051	-0.073	-0.062	-0.062
• head of household at second follow-up	0.233	-0.132***	-0.158***	-0.065	-0.113**
• have children at second follow-up (including those living apart)	0.453	-0.227****	-0.116*	-0.095	-0.141*
• have children living with them at second follow-up	0.422	-0.303****	-0.210***	-0.148*	-0.213****
• have an extra-marital child by second follow-up	0.339	-0.111*	0.005	-0.021	-0.040

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] The unweighted postprogram sample means indicate the magnitude of the variables for all observations (Job Corps and comparison groups). Corpsmember sample means can be obtained by adding the effects shown in this table to the estimates of Corpsmembers' activities had they not participated in Job Corps (presented in Table III.1).

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tailed test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tailed test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tailed test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tailed test).

TABLE IV.2

ESTIMATES OF OVERALL IMPACTS OF JOB CORPS ON FAMILY COMPOSITION

Variable	(1) Estimated Postprogram Sample Mean For All Enrollees	(2) Estimated Sample Mean For All Enrollees Had They Not Entered Job Corps	(3) Average Job Corps Effect For All Enrollees (1) - (2)	(4) Estimated Percentage Impact for All Enrollees (3) ÷ (2) x 100
Probability . . .				
• married at second follow-up	0.106	0.144	-0.038	-26%
• married at second follow-up given never married at pre-enrollment	0.103	0.149	-0.046	-31%
• head of household at second follow-up	0.224	0.268	-0.044	-16%
• have children at second follow-up (including those living apart)	0.225	0.286	-0.062	-21%
• have children living with them	0.137	0.206	-0.069***	-33%***
• have an extramarital child by second follow-up	0.191	0.216	-0.025	-12%

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

TABLE IV.3

ESTIMATES OF CORPSWOMEN'S ACTIVITIES HAD THEY NOT PARTICIPATED IN JOB CORPS[a]

Variables	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
A. FEMALES WITH CHILDREN, NETTING OUT JOB CORPS EFFECTS WITH STANDARD EQUATIONS				
A. Employment and Earnings				
• Employed (fraction of time)	0.223	0.213	0.308	0.284
• Hours worked per week	7.94	7.35	10.65	9.92
• Earnings per week (dollars)	23.55	23.22	31.57	30.12
B. Education and Training				
• In high school (fraction of time)	0.019	0.011	0.010	0.005
• In college (fraction of time)	<0.001	<0.001	0.027	0.006
• In training program (fraction of time)	0.036	0.024	<0.001	<0.001
C. Receipt of Public Assistance				
• Any financial assistance (fraction of time)	0.458	0.547	0.628	0.544
• AFDC (fraction of time)	0.461	0.550	0.606	0.517
• General Assistance or other (fraction of time)	0.296	0.462	0.473	0.451
D. Criminality				
• Total number of arrests per six months	0.079	0.014	0.016	0.048
• Number of theft arrests per six months	0.054	0.017	0.017	0.047
B. FEMALES WITH CHILDREN, NETTING OUT JOB CORPS EFFECTS WITH EQUATIONS CONTROLLING FOR FERTILITY CHARACTERISTICS				
A. Employment and Earnings				
• Employed (fraction of time)	0.183	0.173	0.267	0.243
• Hours worked per week	6.31	5.73	8.98	8.22
• Earnings per week (dollars)	17.58	17.25	25.42	23.84
B. Education and Training				
• In high school (fraction of time)	0.015	0.007	0.006	0.002
• In college (fraction of time)	0.004	<0.001	0.035	0.014
• In training program (fraction of time)	0.025	0.013	<0.001	<0.001
C. Receipt of Public Assistance				
• Any financial assistance (fraction of time)	0.472	0.561	0.642	0.558
• AFDC (fraction of time)	0.467	0.557	0.613	0.523
• General Assistance or other (fraction of time)	0.302	0.469	0.479	0.457
D. Criminality				
• Total number of arrests per six months	0.080	0.015	0.017	0.049
• Number of theft arrests per six months	0.083	0.016	0.016	0.045

TABLE IV.3 (continued)

Variables	0 to 6 Months After Termination	6 to 12 Months After Termination	12 to 18 Months After Termination	18 to 24 Months After Termination
C. FEMALES WITHOUT CHILDREN, NETTING OUT JOB CORPS EFFECTS WITH STANDARD EQUATIONS				
A. Employment and Earnings				
• Employed (fraction of time)	0.322	0.292	0.355	0.370
• Hours worked per week	10.80	11.01	13.41	13.95
• Earnings per week (dollars)	22.55	29.27	38.60	41.31
B. Education and Training				
• In high school (fraction of time)	0.073	0.068	0.055	0.048
• In college (fraction of time)	0.007	0.010	0.012	0.024
• In training program (fraction of time)	0.018	0.023	0.033	0.033
C. Receipt of Public Assistance				
• Any financial assistance (fraction of time)	0.176	0.200	0.185	0.163
• AFDC (fraction of time)	0.134	0.157	0.147	0.130
• General Assistance or other (fraction of time)	0.043	0.042	0.037	0.032
D. Criminality				
• Total number of arrests per six months	0.036	0.024	0.026	0.019
• Number of theft arrests per six months	0.031	0.031	0.015	<0.001

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficient adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] All of the dependent variables in this table are estimated for the civilian population only.

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tailed test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tailed test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tailed test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tailed test).

additional, indirect Job Corps impacts on employment and related activities for females. Since the estimates of Job Corps impacts presented in Chapter III do not take account of this change in family composition, they are likely to underestimate Job Corps impacts on employment and related activities. If additional follow-up interviews are undertaken, the effects of Job Corps on family formation and fertility would need to be modeled and studied more carefully as the sample ages.

B. ESTIMATES OF EMPLOYMENT AND RELATED IMPACTS FOR FEMALES WITH CHILDREN

In general, for females with children we find significant increases in participation in training programs, significant reductions in the receipt of AFDC, and significant reductions in criminality (see Table IV.4 and Figures IV.1 to IV.3). The estimated effects on employment and earnings are small in magnitude and are not statistically significant. As can be seen from Table IV.4, the employment and earnings impacts also vary quite substantially on the basis of whether or not we control for fertility characteristics (i.e., the number of children, presence of extramarital children, and age at the birth of the first child). However, the significant effects on training, AFDC, and criminality are more robust between the two models. With our simple empirical approach it is unclear which underlying model is best; thus, a more structured modeling approach is needed in order to be more definitive. Adding the fertility variables increases the precision of estimates but is also likely to impart some bias (i.e., part of the Job Corps effects will be picked up by these fertility variables, as discussed above). With both sets of estimates, the effects for early dropouts are set equal to zero because, as mentioned previously, we have too few observations to make estimates for this group. When early dropouts are included, the overall effects are not changed substantially,

TABLE IV.4

ESTIMATES OF JOB CORPS IMPACTS ON EMPLOYMENT AND RELATED ACTIVITIES OF FEMALES WITH CHILDREN

(ALL CIVILIANS)

Variables	Unweighted Postprogram Sample Mean[a/]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
		A. JOB CORPS EFFECTS FROM STANDARD EQUATIONS			
A. Employment and Earnings					
• Employed (fraction of time)	0.249	-0.063***	-0.047**	-0.009	-0.029
• Hours worked per week	8.99	-2.04**	-1.27	0.17	-0.61
• Earnings per week (dollars)	27.04	-5.64*	-4.43	-0.82	-3.11
• Weeks worked per six months	6.48	-1.65***	-1.23**	-0.23	-0.77
B. Education and Training					
• In high school (fraction of time)	0.019	-0.011	0.001	-0.003	-0.005
• In college (fraction of time)	0.019	0.006	0.007	0.013*	0.016*
• In training program (fraction of time)	0.033	0.004	0.023***	0.028****	0.018*
C. Receipt of Public Assistance					
• Any financial assistance (fraction of time)	0.381	-0.139****	-0.066***	-0.046*	-0.0002
• AFDC (fraction of time)	0.358	-0.160****	-0.091****	-0.062***	-0.018
• General Assistance or other (fraction of time)	0.023	0.020**	0.024***	0.016*	0.019**
D. Criminality					
• Total number of arrests per six months	0.009	-0.013*	-0.007	-0.016*	-0.019*
• Number of theft arrests per six months	0.006	-0.018***	-0.013*	-0.017***	-0.017**
	B. JOB CORPS EFFECTS FROM EQUATIONS CONTROLLING FOR FERTILITY CHARACTERISTICS				
A. Employment and Earnings					
• Employed (fraction of time)	0.249	-0.024	-0.008	0.032	0.012
• Hours worked per week	8.99	-0.42	0.35	1.84*	1.09
• Earnings per week (dollars)	27.04	0.32	1.55	5.32*	3.16
• Weeks worked per six months	6.48	-0.63	-0.21	0.82	0.30
B. Education and Training					
• In high school (fraction of time)	0.019	-0.008	0.005	0.001	-0.001
• In college (fraction of time)	0.019	-0.002	-0.002	0.006	0.008
• In training program (fraction of time)	0.033	0.015	0.034****	0.040****	0.029***
C. Receipt of Public Assistance					
• Any financial assistance (fraction of time)	0.381	-0.153****	-0.081***	-0.061**	-0.014
• AFDC (fraction of time)	0.358	-0.166****	-0.098****	-0.069***	-0.024
• General Assistance or other (fraction of time)	0.023	0.013	0.017*	0.009	0.012

TABLE IV.4 (continued)

Variables	Unweighted Postprogram Sample Mean[a]	Job Corps Effects 0 to 6 Months After Termination	Job Corps Effects 6 to 12 Months After Termination	Job Corps Effects 12 to 18 Months After Termination	Job Corps Effects 18 to 24 Months After Termination
D. Criminality					
• Total number of arrests per six months	0.009	-0.014*	-0.008	-0.017*	-0.019*
• Number of theft arrests per six months	0.006	-0.018***	-0.013*	-0.016**	-0.016*

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficient adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] The unweighted postprogram sample means indicate the magnitude of the variables for all observations (Job Corps and comparison groups). Corpsmember sample means can be obtained by adding the effects shown in this table to the estimates of Corpsmembers' activities had they not participated in Job Corps (presented in Table III.1).

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tailed test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tailed test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tailed test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tailed test).

FIGURE IV.1
ESTIMATES OF TIME PATHS OF INCREASES IN PERCENT OF TIME EMPLOYED FOR FEMALS WITH CHILDREN[a]

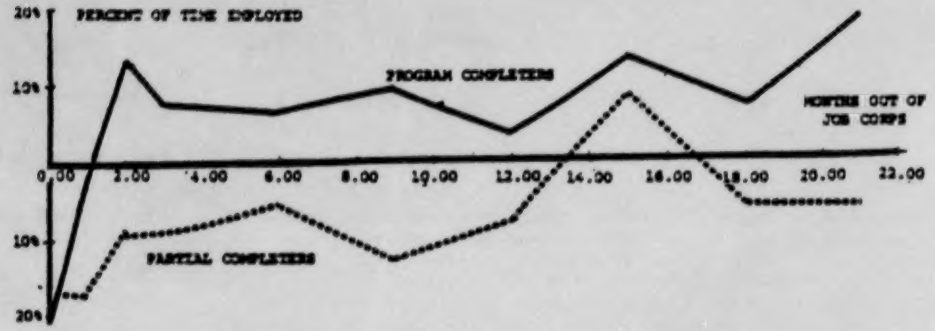


FIGURE IV.2
ESTIMATES OF TIME PATHS OF INCREASES IN EARNINGS FOR FEMALES WITH CHILDREN[a]

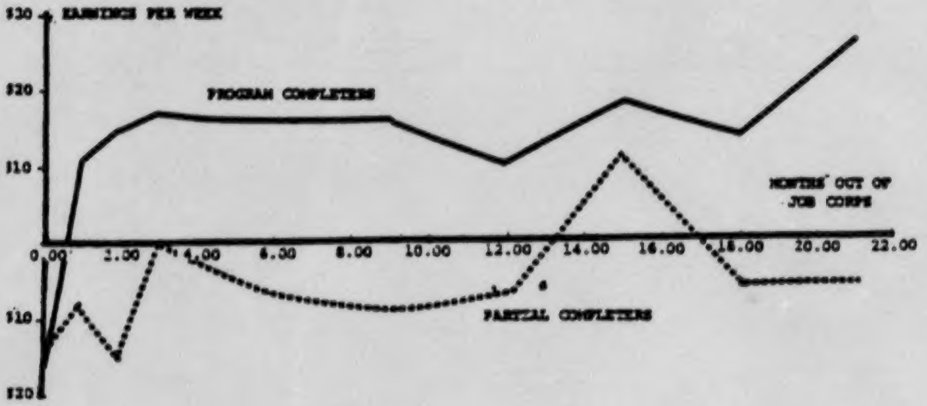
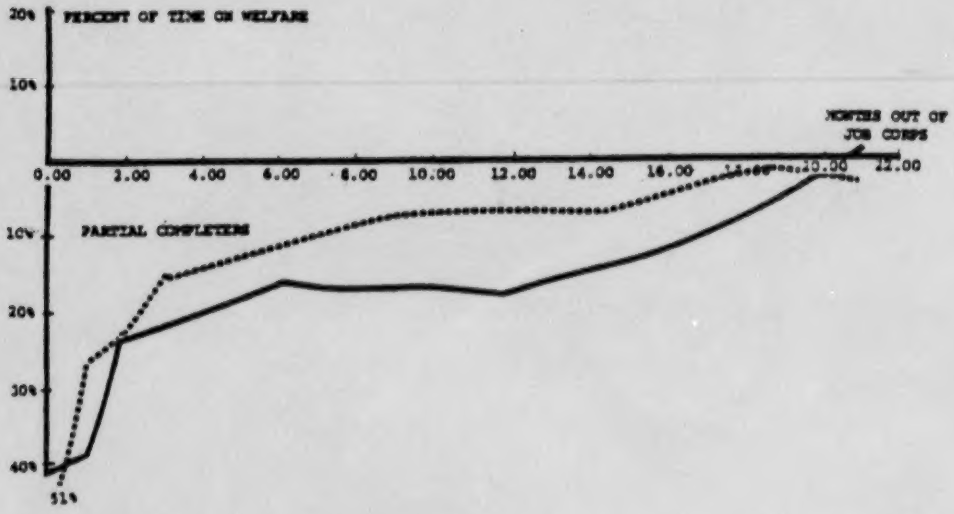


FIGURE IV.3
ESTIMATES OF TIME PATHS OF REDUCTION IN PERCENT OF TIME ON WELFARE FOR FEMALES WITH CHILDREN[a]



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[a] Based on the control specification that includes fertility characteristics.

but the estimates of the timing of effects (i.e., the postprogram time patterns) become extremely erratic.

As can be seen in Table IV.4, setting the effects equal to zero for females with children, as was done in Chapter III, will not substantially bias the estimates of overall Job Corps impacts, especially because this group accounts for only 7 percent of Corpmembers during the first two years of postprogram observation. If findings in Table IV.4 were added to those presented in Chapter III with a weight of 0.07, the only substantive influences would be a slightly larger increase in training-program participation and slightly larger reductions in AFDC and criminality. However, it is expected that the influence of the subgroup of females with children will become more substantial if additional follow-up interviews are undertaken, because this subgroup will continue to grow in both size and relative importance as the sample ages.

C. SUMMARY OF FINDINGS

The findings in this chapter reinforce our estimates of the overall impacts and suggest, if anything, that our overall estimates of Job Corps impacts presented in Chapter III are biased downward by ignoring (1) the Job Corps effects on family composition, and (2) the employment and related effects for females with children. Furthermore, finding delays in family formation for females indicates that this would be a fruitful area for further research, both for the Job Corps program and for other employment and training programs. Finally, if additional follow-up surveys are undertaken, the areas of analysis explored in this chapter will take on increasing importance and will need to be studied more carefully.

V. COMPARATIVE EVALUATION OF BENEFITS AND COSTS[1]

An issue that should be addressed in program evaluations is whether the beneficial effects of the programs outweigh the costs. Answering this question is rarely a simply matter, however--especially in an evaluation of a program such as Job Corps, which has such a wide range of impacts occurring over a long period of time. The effects of Job Corps, for example, include the increased employability of Corpsmembers and its impact on national output, dependence on welfare and other public transfers, criminal activity, drug and alcohol abuse, and the use of alternative training and educational services. Some aspects of these impacts are difficult to value, and many could potentially occur over a long period of time for both Corpsmembers and the rest of society. Comparing these effects of Job Corps to the corresponding costs requires not only that the various benefit and cost components be identified and measured, but that a suitable method be developed for placing relative values on these components.

Benefit-cost analysis attempts to provide an appropriate framework for comparing these effects and their costs. The usual approach entails assigning a current dollar value to each benefit and cost, and aggregating these values by using standard accounting procedures. By measuring the benefits and costs of a program in common units (current dollars), the economic desirability of the program can readily be assessed.

An appropriate procedure in comparing benefits and costs is to calculate the program's "net present value." This term refers to the difference between

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[1] This chapter is a summary of the benefit-cost analysis of Job Corps presented in "Comparative Evaluation of the Benefits and Costs of Job Corps After Eighteen Months of Postprogram Observation" (Technical Report K). The interested reader should refer to that report for a more detailed presentation of the methodology and the findings.

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benefits and costs for which all dollar values have been adjusted to present value units.[1] To control partially for program size, we divide all dollar values by the number of Corpsmembers, so that all figures reflect benefits or costs per Corpsmember. Thus, the resulting criterion used to judge the program is whether the program's net present value per Corpsmember is greater than zero. If it is, the program is "desirable" on economic grounds. If the net present value is less than zero, the program is judged to have been unsuccessful as an economic investment, because the estimated current value of benefits is less than the current value of costs.

While the net present value criterion is easy to state, the degree of uncertainty surrounding its estimation is often high, making it difficult to apply. Sometimes, in fact, equally plausible estimates of a program's net present value may lie on opposite sides of zero, making it impossible either to directly apply the net present value rule or to judge the economic efficiency of a program.

This uncertainty stems from three sources: the estimates of program outcomes, the estimates of the "shadow prices" used to value the outcomes, and the assumptions underlying the evaluation procedures.[2] Of these three areas, the uncertainty surrounding the outcome estimates is best understood because the estimates are derived using statistical methods which yield reliable estimates of error variance. Less is known about the accuracy of the shadow prices. These prices are estimated on the basis of published data for which standard measures of error or uncertainty

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[1] In discounting to present value units, we adjust the value of estimated benefits or costs that accrue in the future to reflect their worth in the current time period.

[2] The term shadow price is used to refer to estimates of the value per unit of the effects. These prices are then multiplied by the changes in the outcomes to arrive at their value. This technique is discussed in the next section.

(e.g., standard deviations or ranges) are often not available. Finally, many of the assumptions used in the analysis clearly approximate reality--the magnitude of the approximation error often being impossible to determine precisely.[1]

Because of the error associated with any single estimate of net present value, much of the usefulness of benefit-cost analysis comes from the comprehensiveness of the analysis. The process of drawing together measures of the various inputs and outcomes and the general patterns that emerge from the attempts to assign relative values are often more useful than any specific estimate of net present value. For this reason, the analysis does not focus on a single net present value estimate but, instead, on a set of estimates. This set includes (1) a benchmark estimate, incorporating the assumptions and estimates with which we feel most comfortable, and (2) several estimates based on sensitivity tests, each illustrating the effect of changing one or more of the assumptions used in the benchmark calculations while holding the others constant.

The conclusions of the benefit-cost analysis are based on all these estimates. Thus, they do not rely on a single set of uncertain assumptions and estimates, but, instead, on a range of plausible assumptions and estimates. By examining the different assumptions, the underlying outcome estimates, and the techniques used to value outcomes, reasonable judgments can be made about the relative value of program benefits and costs.

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[1] The relative importance of these types of errors is not well known. However, it seems likely that uncertainty surrounding the validity of assumptions, such as the appropriate rate of discount, causes the greatest uncertainty regarding the true net present value.

For Job Corps, the benchmark estimate of social net present value is almost $2,000 per Corpsmember, measured in 1977 dollars. This, along with the generally positive results of the sensitivity tests, suggests that Job Corps is a good investment for society.

While the evaluation of the program from the perspective of society as a whole is positive, all groups in society do not share the benefits and costs equally. It is therefore important to consider the effect of the program on the distribution of resources, as well as its effect on the total amount of resources. In economics, these two concerns of the analysis are referred to as "equity" and "efficiency" issues. Efficiency concerns a program's effect on the total value of the goods and services available to society (Is the value of those goods and services greater because of the program under study, or would it have been greater if the resources used for the program had been devoted to alternative uses?). Equity concerns the distribution of goods and services among groups in society and how the distribution is affected by the program.

To address these two policy questions, the benefit-cost analysis estimates the net present value for three key perspectives: society as a whole, Corpsmembers, and non-Corpsmembers.[1] The benefit-cost analysis will be limited to estimating the magnitude of any distributional changes without drawing conclusions about their desirability. As might be expected, given the nature of Job Corps, we find an overall transfer from non-Corpsmembers to Corpsmembers.[2]

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[1] The term non-Corpsmember is used consistently throughout this benefit-cost discussion to refer to all members of society other than those who enroll in Job Corps. It should be pointed out that this term is not meant to refer specifically to that group of non-Corpsmembers interviewed in our study as a comparison group, although these individuals are of course included as a small fraction of the total non-Corpsmember group.

[2] Since the net present value to society is positive, however, presumably everyone could be made better off than they would be without the program.

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One analytically useful feature of evaluating Job Corps from these three perspectives (society, Corpsmembers, and non-Corpsmembers) is that the sum of the net present values calculated from the Corpsmember and non-Corpsmember perspectives equals the social net present value--that is, Corpsmembers and non-Corpsmembers together include all members of society. Therefore, transfers between these two groups (e.g., reduced welfare transfers or Job Corps stipends) cancel each other out when the net present values are summed, and thus do not appear in the social net present value.[1] Benefits or costs that accrue to one group and are not offset by corresponding costs or benefits to the other group (e.g., increased work output) will not cancel out, and thus will enter into the social net present value calculation.

The components of our benefit-cost analysis of Job Corps and the relationships among the Corpsmember, non-Corpsmember, and social perspectives are illustrated in Table V.1. This table lists the principal components of the benefit-cost analysis, suggests whether a component is, on average, a benefit, a cost, or neither from each of the three perspectives, and indicates the data sources used to measure and value each component.[2] The individual benefit-cost components listed in Table V.1 are the subject of the following section.

Finally, before proceeding, it is important to note the general approach adopted in our evaluation. In general, impacts and costs are valued using an accounting framework based on the during-program period of 1977, and by estimating the market value of the resources saved or used

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[1] This assumes that a dollar of benefit or cost to one person is equal to a dollar of benefit or cost to anyone else.

[2] Whether the effect of Job Corps on a component is a net benefit or cost is sometimes problematic. Table V.1 reflects prior judgments concerning the social value of components. The treatment of all components in the final net present value calculations is, of course, determined by the measures of the actual outcomes.

TABLE V.1

COMPONENTS OF BENEFIT-COST ANALYSIS

Component	Perspective[a]			Data Source(s)
	Social	Non-Corpsmember	Corpsmember	
BENEFITS				
A. Output Produced by Corpsmembers				
• In-program output	+	+	+	S,P
• Increased postprogram output	+	0	+	I,P
• Increased tax payments on postprogram income	0	+	-	I,P
• Increased utility due to preferences for work over welfare	+	+	+	N
B. Reduced Dependance on Transfer Programs				
• Reduced transfer payments	0	+	-	I,P
• Reduced administrative costs	+	+	0	I,P
C. Reduced Criminal Activity				
• Reduced criminal justice system costs	+	+	0	I,P
• Reduced personal injury and property damage	+	+	0	I,P
• Reduced value of stolen property	+	+	-	I,P
• Reduced psychological costs	+	+	+	N
D. Reduced Drug/Alcohol Use				
• Reduced drug-treatment costs	+	+	0	I,P
• Reduced alcoholism-treatment costs	+	+	0	I,P
• Increased utility from reduced drug/alcohol dependance	+	+	+	N
E. Reduced Utilization of Alternative Services				
• Reduced costs of training and educational programs other than Job Corps	+	+	0	I,P
• Reduced net costs of Public Service Employment	+	+	0	I,P
• Reduced training allowances	0	+	-	I,P
F. Other Benefits				
• Increased utility from redistribution	+	+	+	N
• Increased utility from improved well-being of Corpsmembers	+	+	+	N
COSTS				
A. Program Operating Expenditures				
• Center operating expenditures, excluding transfers to Corpsmembers	-	-	0	A
• Transfers to Corpsmembers	0	-	+	A
• Central administrative costs	-	-	0	A,S
B. Opportunity Cost of Corpsmember Labor During the Program				
• Foregone output	-	0	-	I,P
• Foregone tax payments	0	-	+	I,P
C. Unbudgeted Expenditures Other than Corpsmember Labor				
• Resource costs	-	-	0	S,P
• Transfers to Corpsmembers	0	-	+	S,P

[a] The columns indicate whether the net impact of a particular item is a net benefit (+), a net cost (-), or neither (0). This is done from the social, non-Corpsmember, and Corpsmember perspectives in order to indicate redistributive effects. In doing this, Corpsmembers are treated as nontaxpayers (except in benefit component A.3 and cost component B.2) to simplify the exposition, and non-Corpsmembers encompass everyone in society other than Corpsmembers.

[b] The codes for data sources are: S = special study; I = interview; P= published data source; A = Job Corps financial accounting system; N = not measured.

because of the program. When market values are not observed directly, the implicit shadow prices will be estimated whenever possible. However, in some cases, reliable shadow prices cannot be estimated. For example, we will not value the increase in social utility due to individuals' preferences for work over welfare.[1] When equally plausible estimates exist, we will adopt the conservative convention of using the value which lowers the estimate of net present value.

A. BENEFIT COMPONENTS

Five major benefit components are measured and valued. All of them are expected to derive, at least in part, from an increase in the long-run employability of Corpsmembers. Improved job opportunities should lead to benefits from increases in the production of goods and services and from reductions in (1) criminal activities, (2) drug and alcohol abuse, (3) welfare dependence, and (4) the use of alternative training and educational services. Our estimates for each of the five components of benefits plus unmeasured benefits are discussed briefly below; they are then aggregated together with the costs, in Section C.

1. Output Produced by Corpsmembers

The increase in goods and services produced by Corpsmembers constitutes a major benefit of the program. For analytical purposes, it is useful to distinguish between goods and services that Corpsmembers produce while they are enrolled in Job Corps and those that they produce after they leave the program. This distinction is convenient because different techniques are needed for valuing changes in postprogram versus in-program output. The output produced by Corpsmembers after they leave the program is produced in the regular labor market, and we estimate the value of this output on the basis of their wages. In contrast, the output produced by Corpsmembers while enrolled in Job Corps is produced under nonmarket circumstances, and their Job Corps stipends (pay allowances) do not provide an accurate index of the value of that output. (For similar reasons, the different types of output are treated differently in the accounting frameworks for the three benefit-cost perspectives.)

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[1] One way to interpret quantitative benefit-cost findings when some benefits and costs are not measured is that if measured costs exceed measured benefits, society (or another perspective) must value the difference between unmeasured benefits and costs by at least the amount of the measured shortfall in the net present value in order for the program to be considered a worthwhile economic investment.

In-Program Output. The in-program output produced by Corpsmembers in connection with their vocational training provides benefits to Corpsmembers, to non-Corpsmembers, and to society as a whole.[1/] These outputs include goods produced in work projects (for instance, the addition built onto a hospital in rural Colorado by Corpsmembers who were receiving on-the-job training in various construction trades) and services provided in work-experience programs (for instance, the nursing assistance provided by Corpsmembers at a county hospital in Guthrie, Oklahoma, as they were gaining job experience). The value of these goods and services was estimated on the basis of twenty-two special studies of randomly chosen work projects and work-experience programs at eleven Job Corps centers.

The recipients of this Corpsmember-produced output may be either the non-Corpsmember community or the Job Corps centers themselves. In the first case (community-serving output), the entire value of the output

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[1/] For more details regarding the estimated value of in-program output and the techniques used to obtain those estimates, see Technical Reports D and E.

produced is considered a benefit to non-Corpsmembers.[1] In the second case (center-serving output), the output will benefit both Corpsmembers and non-Corspmembers. Corpsmembers benefit from center-serving output because they consume some of the output they produce (e.g., housing services provided in dormitories built or rehabilitated with Corpsmember labor, part of which is included as a capitalized cost in the Job Corps financial data); non-Corpsmembers benefit when the capital stock available to society is increased by Corpsmember labor in these activities.

The value of the goods and services produced by Corpsmembers in community- and center-serving projects is estimated by the price that alternative suppliers would have charged to provide those goods and services.[2] After adjustments are made to center-serving output (to account for the value of output used up by Job Corps and transferred to Corpsmembers), the net benefits per Corpsmember year of service of in-program output are estimated to be $1,364 for non-Corpsmembers, $175 for Corpsmembers, and therefore $1,539 for society as a whole. Because the average Corpsmember stays in the program approximately half a year (5.9 months in fiscal year 1977), the average social benefit per Corpsmember is $757.[3] On average, Corpsmembers receive $86 of this benefit, while the remaining $671 accrues to non-Corpsmembers.

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[1] Corpsmembers also benefit from the community-serving output as general members of society. However, for the most part, we will use the approximate (and computationally convenient) assumption that only non-Corpsmembers benefit from such output.

[2] The value of all materials and labor inputs provided by Job Corps are subtracted from the alternative supplier's price. In many cases, the net value of the Job Corps output was quite close to the alternative supplier's labor cost.

[3] That is, 5.9 ÷ 12 x $1,538.83 = $756.59.

These values are based on the price charged by an alternative supplier and indicate the value of the resources that would be required to produce the in-program output of Corpsmembers. However, these prices do not directly measure the value that society places on the output. While only imprecise estimates of this demand value can be made, tests presented in Technical Report E suggest that under reasonable assumptions the demand value will be between 103 and 62 percent of the supply-price estimate. Thus, using supply price as a measure of the value of in-program output can provide a reasonably accurate estimate of the demand value (but probably high by a small amount).

Increased Postprogram Output. The increase in the amount of output produced by Corpsmembers after they leave the program is estimated by the increase in earnings.[1/] The use of earnings as a measure of output produced is based on the assumption that labor markets function in a competitive manner (earnings is the correct measure from the Corpsmember perspective in any case). This assumes that employers set the total compensation of a worker at a value that reflects the output produced by the worker. The increase in output produced by Corpsmembers is then estimated by the difference between the gross compensation of the Corpsmember and the amount they would have been paid had they not entered the program.[2/]

The interview data on earnings have two shortcomings with respect to the estimation of total compensation. First, they deal only with the wage component of total compensation and ignore the nonwage components, such as

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[1/] This assumes that Corpsmembers do not displace other workers who subsequently become unemployed (see Technical Report K for more discussion on this topic).

[2/] See the discussion in Chapter III for details regarding the econometric procedures used and the resulting impact estimates obtained for the Job Corps effects that are being valued in this chapter.

retirement, health, and insurance benefits, the employer's share of payroll taxes for Social Security, and payments made into Unemployment Insurance and Workers' Compensation funds. Second, the earnings data per se do not include increases in output due to Corpsmember increases in military service.

Estimates of nonwage benefits were made on the basis of secondary data (e.g., Social Security statutes and Department of Labor estimates of fringe-benefit rates). For workers like the Corpsmembers, we estimate the value of the nonwage items to be 15 percent of wages. Thus, the social benefit derived from the increase in output is estimated at 1.15 times the estimated increase in Corpsmembers' earnings.

The values of increases in military earnings and output were estimated on the basis of the estimates of increased military service among Corpsmembers and of the average compensation paid by the military. Estimated military compensation was based on the fiscal year 1977 regular military compensation rates for grades E-1 ($6,861 per year) and E-2 ($7,470 per year).[1]

Estimates of the total increase in output and its three components--civilian earnings, military earnings, and fringe benefits--are shown in Table V.2 (along with the related estimate of the value of increased tax payments). As discussed in Chapter III, the pattern of results over time suggests that Corpsmembers experience some adjustment problems as they re-enter the labor market after Job Corps. As Corpsmembers overcome the readjustment problems, earnings rise until the semi-annual increase in output is worth $325 per Corpsmember. Increases in military output account for almost half of the output gain during the first 6 months,

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[1] For further details and justification, see Technical Report K.

TABLE V.2

ESTIMATED VALUE OF INCREASED POSTPROGRAM OUTPUT PER CORPSMEMBER

(1977 DOLLARS)

Source of Income	Months 1 to 6	Months 7 to 12	Months 13 to 18	Months 19 to 24	Total Discounted Value[a]
Increased civilian earnings	$ 49.79	$147.89	$229.27	$213.65	$595.80
Increased military earnings	43.53	36.34	72.17	70.90	208.31
Total earnings increase	$ 93.32	$184.23	$301.44	$284.55	$804.10
Increased fringe benefits[b]	14.00	27.63	45.22	42.68	120.61
Value of increased postprogram output	$107.32	$211.86	$346.66	$327.23	$924.72
Increased tax payments[c]	$ 5.14	$ 27.19	$ 55.05	$ 50.12	$127.41

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[a] Discounted to the in-program period at a 5 percent annual rate (for justification, see the text).

[b] Fringe benefits are estimated to be 15 percent of earnings (for justification, see the text).

[c] Tax payments are estimated to be 23 percent of income, earnings plus transfers (for justification, see the text).

but this proportion falls as Corpsmembers adjust to re-entering the civilian labor market. During the last observation period, 19 to 24 months after leaving Job Corps, military output accounts for only approximately 25 percent of the total increase in output. The total estimated present value of increased postprogram output is approximately $925 during the 24-month observation period.

Increased Tax Payments on Postprogram Income. As Corpsmembers' incomes rise they pay more taxes. Such an increase in tax payments is a cost to Corpsmembers, but an offsetting benefit to non-Corpsmembers (i.e., all other taxpayers) and, hence, does not enter the social perspective.[1]

To estimate taxes paid, we used an estimate of the tax rate applicable to low-income households. This rate was estimated by Pechman and Okner (1974) to be approximately 23 percent of total income (i.e., earnings plus transfers).[2] The major components of this rate are payroll, sales, and excise taxes. These taxes, especially those on consumption, are difficult to avoid. Thus, even though Corpsmembers may face low income-tax rates on wages, and may in fact avoid paying payroll and income taxes, their total tax burden (as a percentage of income) is not significantly different than the tax burden of most taxpayers (although the composition of taxes does vary considerably by income level).

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[1] As is the case with all transfers, changes in the resource costs of making the transfer should be included in the social perspective. In the case of tax payments, the change in administrative costs is probably very small and is treated as zero.

[2] This discussion draws on the results of Pechman and Okner (1974). Their data show that the combined effect of all taxes is equivalent to a proportional tax of approximately 23 percent of income. These data are rather old--1966--and have some inaccuracies when applied to populations of low-income youths. However, they are the best estimates currently available--particularly because of their comprehensiveness and because they are able to incorporate the effects of tax avoidance in their estimates.

The pattern of changes in tax payments (shown in Table V.2) mirrors that of increased earnings.[1] During the first 6 months after leaving Job Corps, Corpsmembers pay, on average, only about $5 more in taxes than they would have paid in the absence of the program. This increase in taxes rises as earnings rise, and during the last observation period (months 19 to 24) they pay approximately $50 more than they would have in the absence of the program.

Unmeasured Benefits. In addition to the increases in output and tax payments, there are also gains for both Corpsmembers and non-Corpsmembers to the extent that individuals prefer work over welfare. Corpsmembers may gain from increases in self-esteem due to working in regular, unsubsidized jobs. Non-Corpsmembers may benefit to the extent that they would prefer the resources to be used to give Corpsmembers the opportunity to increase their human capital and earnings from Job Corps participation, rather than be used to provide direct transfer payments to Corpsmembers. These changes in well-being are intangible and could not be measured accurately for this analysis.

2. Reduced Dependence on Transfer Programs

Increases in employability attributed to Job Corps should make participants less reliant on transfer programs. This will cause a decline in both transfer payments to Corpsmembers and the level of resources needed to administer those programs. The reductions in transfer payments represent a cost to Corpsmembers and a corresponding benefit for non-Corpsmembers, who otherwise would have had to pay for them. Therefore, these transfer payments net out from the social perspective. In contrast, the administrative savings are a benefit to non-Corpsmembers and do not represent a corresponding cost to Corpsmembers; hence, the administrative savings are a benefit to society.

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[1] Because there were reductions in the amount of transfer payments to Corpsmembers, the change in taxes (figured on the basis of changes in earnings and transfers) is less than it would have been if figured only on the basis of earnings increases shown in Table V.2. These reductions in transfers are discussed in the next section of this chapter.

Reduced Transfer Payments. Six transfer programs were examined as part of the analysis: Aid to Families with Dependent Children (AFDC), General Assistance, Food Stamps, Medicaid, Unemployment Insurance, and Worker's Compensation.[1] Transfers were estimated on the basis of differences in months during which the transfers were received and the average benefit levels of the programs. These benefit levels were estimated on the basis of published data, except in the cases of Unemployment Insurance and Workers' Compensation, for which the average benefit received by persons in our sample was used (a smaller amount than the average benefit levels of the programs).

Corpsmembers reduced their participation in all six programs both while they were in Job Corps and during the first 24 months after they had left the program. The largest reductions were in the cash

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[1] In addition to these six programs we also examined the use of public housing. The principal reason why changes in the use of this program are not included in the analysis is that almost all of the persons who reported living in public housing said that they lived with parents or other relatives. Thus, the fact that they moved out of public housing would not necessarily imply any change in public housing subsidies or administrative costs, because the parents and relatives could continue to live in the housing unit. In any event, changes in the use of public housing were quite small, and the estimated error introduced by not including this program is at most $15 per Corpsmember.

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welfare programs, AFDC, and General Assistance (for valuation purposes, Medicaid benefits were estimated on the basis of AFDC participation). There was also a sizable reduction in the receipt of Unemployment Insurance. Estimates of the changes in transfer payments by program are shown in Table V.3 and total approximately $527 in present value terms during the observation period.

Reduced Administrative Costs. With the decline in transfers, the amount of resources needed to administer the programs also declines. This resource saving is a benefit to non-Corpsmembers and to society as a whole. The savings are estimated on the basis of the estimated changes in months of program use and the average costs per month of processing a case in each of the programs. The estimated benefits are presented in Table V.4 and total approximately $63 in present value terms during the observation period.

3. Reduced Criminal Activity[1]

Four benefits from the decline in criminal activity among Corpsmembers are the reductions in (1) the resources used in the criminal justice system; (2) the personal injury and property damage that accompany victimizations; (3) the value of stolen property; and (4) the fear and anxiety associated with crime. The resource savings associated with the first two items are benefits to society and to non-Corpsmembers. The reduced value of stolen property will be a benefit to non-Corpsmembers, but part of its value should be viewed as a cost to Corpsmembers, who no longer receive that theft income. The social benefit of a reduction in

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[1] This section is, by necessity, a brief summary of the crime evaluation procedures used. Technical Report K contains a more complete discussion of the procedures and their justifications.

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TABLE V.3

ESTIMATED VALUE OF BENEFITS PER CORPSMEMBER FROM REDUCED TRANSFER PAYMENTS

(1977 DOLLARS)

	Months of Reduction in Use						
		Postprogram Period					
Program	In-Program Period	Months 1 to 6	Months 7 to 12	Months 13 to 18	Months 19 to 24	Value Per Month	Total Discounted Value[a]
Aid to Families with Dependent Children	0.572	0.166	0.185	0.200	0.215	$76.41	$99
General Assistance	0.059	0.158	0.146	0.123	0.117	$116.05	66
Medicaid	0.572	0.166	0.185	0.200	0.215	$76.61	99
Food Stamps	0.482	0.135	0.122	0.082	0.050	$73.65	63
Unemployment Insurance	-	0.128	0.146	0.128	0.119	$63.43	135
Workers' Compensation	-	0.023	0.012	0.029	0.018	$109.43	37
Unemployment insurance and Workers' Compensation	$29.30[b]	-	-	-	-	-	29
Total benefit							$527

[a] Discounted to the in-program period at a 5 percent annual rate (for justification, see the text).

[b] For the in-program period, Workers' Compensation (WC) and Unemployment Insurance (UI) had to be combined because of data limitations with the Baseline Questionnaire. The estimated value is taken from interview self-reports of the total amount of WC and UI combined.

TABLE V.4

ESTIMATED VALUE OF BENEFITS PER CORPSMEMBER FROM REDUCED ADMINISTRATIVE COSTS OF TRANSFER PAYMENTS

(1977 DOLLARS)

	Months of Reduction in Use						
		Postprogram Period					
Program	In-Program Period	Months 1 to 6	Months 7 to 12	Months 13 to 18	Months 19 to 24	Value Per Month	Total Discounted Value[a]
Aid to Families with Dependent Children	0.572	0.166	0.185	0.200	0.215	$8.83	$11
General Assistance	0.059	0.158	0.146	0.123	0.117	13.35	8
Medicaid	0.572	0.166	0.185	0.200	0.215	9.49	12
Food Stamps	0.482	0.135	0.122	0.082	0.050	9.48	8
Unemployment Insurance[b]	0.144	0.128	0.146	0.128	0.119	30.16	19
Workers' Compensation[b]	-	0.023	0.012	0.029	0.018	53.73	4
Total benefit							$63

[a] Discounted to the in-program period at a 5 percent annual rate (for justification, see the text).

[b] For the in-program period, Workers' Compensation and Unemployment Insurance are combined. The Unemployment Insurance costs are used because participation in that program is much greater than participation in Workers' Compensation.

stolen property (the difference between the non-Corpsmember benefit and the Corpsmember cost) is the decrease in the costs of fencing, in damage to the stolen property, and in the loss of legal titles.

The method used to value the crime-reduction benefits focuses on the effect of changes in arrests among nine crime categories. The estimates of Job Corps-induced reductions in arrests are adjusted upward by 70 percent to correct for underreporting in the interview self-reports.[1] These estimated reductions in arrests were then valued by multiplying them by shadow prices equal to the cost savings per arrest. The disaggregation of arrests was made by most serious charge, which will enable changes in both the mix of arrest charges and the overall level of arrests to be valued. The shadow prices applicable to the social perspective are presented in Table V.5.

Reduced Criminal Justice System Costs. Processing persons through the criminal justice system (police, prosecution, courts, corrections) is expensive. Court time alone is estimated to cost $15 per minute, so that even the simple process of entering a plea costs close to $450.[2] The prices in the table reflect the probability and cost of an arrested person passing through each stage of the system--police custody, arraignment, detention, trial, and incarceration.

Reduced Personal Injury and Property Damage. Another major benefit associated with reduced criminal activity is the decrease in victimizations. The victimization benefits included in this analysis

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[1] Schore, Maynard, and Piliavin (1979) found that arrests were underreported by between 41 and 48 percent when interview questions (essentially the same as those used in this analysis) were compared to official court records. Thus, the self-reports must be multiplied by 1.7 to obtain an estimate of the actual number of arrests. For further details, see Technical Report K.

[2] Greenwood et al.'s (1973) estimate adjusted for price inflation.

TABLE V.5

ESTIMATES OF AVERAGE SOCIAL COST OF CRIME PER ARREST, BY ARREST CHARGE

(1977 DOLLARS)

Arrest Charge	Criminal Justice System Costs	Personal Injury and Property Damage Costs[a/]	Stolen Property Resource Costs[b/]	Total Measured Cost Per Arrest
Murder	$24,767	$100,538	$ 0	$125,305
Felonious assault	2,732	489	0	3,221
Robbery	12,087	569	497	13,135
Burglary	5,895	537	2,317	8,479
Larceny and motor vehicle theft	2,618	408	1,268	4,294
Drug law violation	2,590	0	0	2,590
Other personal crimes	756	94	0	850
Other miscellaneous crimes	919	0	0	919
Unspecified crimes[c/]	2,048	171	348	2,567

[a/] The drug-law violations and other-miscellaneous-crimes categories contain primarily "victimless" crimes; hence, a value of zero is assumed.

[b/] Stolen property resource costs, estimated only for property crimes, are estimated as a fraction (65 percent) of the average value of property stolen per arrest (see the text and Technical Report K for details).

[c/] The unspecified-crimes category contains arrests for which the arrest charge was either not recorded or undecipherable. Costs for this category are estimated as the weighted average of the costs of the other crime categories.

are the resource savings from reductions in the amount of personal injury and property damage. Using data collected as part of the National Crime Panel Survey program, we estimated (1) the average value of property damage from criminal acts, (2) the average value of the medical care needed by victims of personal crimes, (3) the average output lost when victims lose time from work while they are recovering from personal crimes, and (4) the average costs of administering insurance with which to compensate victims.[1] The average cost per victimization figures were multiplied by the ratio of victimizations to arrests in order to represent the average cost per arrest.

Reduced Value of Stolen Property. Estimates for the value associated with the reduction in stolen property were obtained from vicitimization data in a manner similar to that used to estimate the cost of reductions in property damage and personal injury. The major difference is that part of the value of stolen property represents a transfer from victim to thief. The remaining part of the value of stolen property is the social cost associated with fencing the goods, with a decline in the value of the goods because they cannot be sold with a legal title, or with a decline in value because of damage. The relative magnitudes of these components are estimated on the basis of a study which found that thieves were able to realize only 35 percent of the value to victims when they converted stolen goods into cash.[2]

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[1] We would like to thank Wesley G. Skogan for his help in obtaining the necessary estimates from the victimization-incident data that were gathered as part of the National Crime Panel program. Technical Report K presents a detailed breakdown and analysis of the average costs of victimizations by category of crime.

[2] U.S. Drug Enforcement Administration, Heroin-Related Crimes (1977). The 35 percent estimate takes account of the fact that stolen cash and other liquid assets do not need to be fenced.

Thus, non-Corpsmembers are assumed to view the full reduction in stolen property as a benefit, while Corpsmembers view 35 percent of the value of that reduction as a cost. The remaining 65 percent of the reduced value of stolen property constitutes a social benefit.

Reduced Psychological Costs. The values presented above capture only part of the costs of criminal victimizations. In particular, they fail to capture completely the emotional benefits individuals derive from reductions in crime. These benefits are undoubtedly important; however, because there is no accurate way to estimate their magnitude, they have not been included in the numerical estimates. Their exclusion will bias our benefit-cost estimates downward, and they must be kept in mind when interpreting the results.

The Value of Social Benefits from Job Corps Reductions in Crime. Table V.6 presents the estimates of the Job Corps-induced changes in arrests for the nine arrest types. These figures have been adjusted for underreporting. The social shadow prices (i.e., the average social costs from Table V.2) of the different arrest types have been entered in the next to last column. The final column gives the total discounted value of the crime benefits for the in-program period and the first 24 months after leaving Job Corps. The largest benefits are for reductions in property crimes, robbery, burglary, and larceny. The reductions are concentrated in the in-program period and appear to fade out quickly over time. Altogether, the social net value of the reduction in arrests is estimated to be worth almost $2,000 per Corpsmember over this period. When the redistributional aspects of stolen property are taken into account, we estimate a $2,115 per Corpsmember benefit for non-Corpsmembers, and an average cost to Corpsmembers of $153.

TABLE V.6

ESTIMATED VALUE OF SOCIAL BENEFITS PER CORPSMEMBER FROM REDUCED CRIMINAL ACTIVITY

(1977 DOLLARS)

	Reductions in Arrests per Corpsmember						
		Postprogram Period					
Arrest Charge[a]	In-Program Period	Months 1 to 6	Months 7 to 12	Months 13 to 18	Months 19 to 24	Social Value Per Arrest	Total Discounted Value[b]
Murder	0.002	0.001	0.001	0.001	0.001	$125,305	$636
Robbery	0.002	0.006	0.002	0.004	0.004	13,135	223
Felonious assault	0.005	-0.001	-0.004	0.000	-0.011	3,221	-31
Burglary	0.052	0.001	0.001	0.001	0.005	8,479	517
Larceny/motor vehicle theft	0.059	0.012	0.011	0.009	0.016	4,294	448
Drug law violation	0.026	0.003	0.002	-0.001	-0.005	2,590	63
Other personal crimes	0.019	0.001	0.003	0.001	0.000	850	20
Other miscellaneous crimes	0.050	-0.001	-0.003	-0.004	-0.013	919	29
Unspecified crimes[c]	-0.003	0.007	0.009	0.010	0.002	2,567	59
Total benefit							$1,962

NOTE: Details do not sum to totals because of rounding.

[a] In those cases where there was more than one arrest charge, only the most serious charge was used.

[b] Discounted to the in-program period at a 5 percent annual rate (for justification, see the text).

[c] The unspecified-crimes category contains arrests for which no charge was recorded. Costs for this category are estimated as the weighted average of the costs for the other crime categories.

4. Reduced Drug and Alcohol Use

The principal measured benefit of the reduction in drug and alcohol use is the decrease in treatment costs. The types of drug-alcohol treatments included in the estimates are residential and nonresidential drug (principally heroin) detoxification, residential and nonresidential "drug-free" treatment, alcohol detoxification, and education and counseling services. The resource savings associated with the reduction in the use of drug-alcohol treatments will benefit both non-Corpsmembers and society as a whole. The emotional benefits from reduced drug and alcohol use, while unmeasured in this report, will accrue to both Corpsmembers and non-Corpsmembers.

In general, there was very little use of drug or alcohol treatment programs by Corpsmembers. The largest effect was observed while they were in the Job Corps. During the postprogram period the decreases in treatment use were all very small (a difference of less than one day in treatment per 6-month period). The present value of the resources saved because of these reductions is estimated to be approximately $31 per Corpsmember for both the in-program period and the first two years of the postprogram period (approximately $22 of this benefit accrues during the in-program period).

5. Use of Alternative Training and Educational Programs

Decisions by Corpsmembers to obtain more or less training and schooling generate benefits and costs to both Corpsmembers and non-Corpsmembers. For example, Corpsmembers enroll in high school programs much less frequently than individuals in the comparison group. This is due, in part, to the fact that many Corpsmembers obtained GED degrees while they were in the program (see Chapter III). In this case, the resource savings associated with less frequent high school enrollment are benefits to non-Corpsmembers and to society. Of course, there are other benefits associated with additional training and education--most importantly, increased lifetime earnings. Ideally, our estimates of increased earnings and other effects should include these benefits for both the Corpsmembers and the individuals in our comparison group. However, the absence of a sufficiently long observation period forces us to measure the changes in the operational costs of education and training programs without observing much of the subsequent benefit. Thus, our estimates of the net present value will be biased downward to the extent that Job Corps induces Corpsmembers to obtain higher levels of education (compared to what they would have obtained in the absence of Job Corps).[1/]

Table V.7 presents the estimated value of changes in Corpsmembers' use of education and training programs; these include the traditional school programs--high school, vocational school, college or university education, and other schooling (mostly adult education programs)--and three employment and training programs--CETA training programs, the Work Incentive Program (WIN), and public service employment programs (primarily CETA). The largest reduction is in the use of high school. Also, there are small reductions in the use of the employment and training programs, especially during the in-program and early postprogram periods. The increase in the use of college and vocational education programs results in a small offsetting cost. The net result is a resource savings of $85 per Corpsmember during the observation period.

[1/] Further discussion regarding the treatment of long-run education and training benefits is presented in Technical Report K.

TABLE V.7

ESTIMATED VALUE OF BENEFITS PER CORPSMEMBER FROM REDUCED UTILIZATION OF ALTERNATIVE TRAINING AND EDUCATIONAL PROGRAMS

(1977 DOLLARS)

	Reduction in Weeks of Program Use						
		Postprogram Period					
Program	In-Program Period	Months 1 to 6	Months 7 to 12	Months 13 to 18	Months 19 to 24	Value Per Week	Total Discounted Value[a]
I. Employment, Training, and Work Experience Programs							
CETA and related training	0.123	0.083	0.021	0.062	0.056	$38	$13
WIN	0.000	0.012	0.017	0.023	0.023	74	5
Public Service Employment (PSE)	0.208	0.085	0.017	-0.042	-0.028	14	3
II. School							
High school	0.548	0.345	0.263	0.282	0.281	46	76
Vocational education	0.092	-0.051	-0.108	-0.064	-0.075	23	-4
College/ university	0.065	-0.080	-0.125	-0.136	-0.163	85	-34
Other school	0.163	0.118	0.083	0.100	0.084	51	27
Total benefit							$85

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[a] Discounted to the in-program period at an annual rate of 5 percent (for justification, see the text).

In addition to the changes in resource use in the various education and training programs, there will also be a transfer resulting from changes in training allowances paid to Corpsmembers. On the basis of interview data, we estimate that Corpsmembers received an average of $49 less in training allowances (other than those from Job Corps) because of their enrollment in Job Corps. Almost half of this reduction is estimated to have taken place while they were in Job Corps.

6. Other Benefits

In addition to the benefit components discussed above, there are two benefits that cannot be directly measured and valued; however, evidence on them does exist. One important benefit that cannot be directly measured is the utility that participants and nonparticipants derive from the income redistribution per se that is implicit in the Job Corps program. The other benefit is the improved personal well-being of participants beyond what is caused by both increased earnings and the value of Job Corps expenditures on enrollees (for room, board, medical services, etc.). In particular, it is very likely that the value of observed improvements in health status and basic education are not fully captured in this analysis.

7. Benefits After the Observation Period

To this point, the discussion of benefits has focused on the time period over which the Corpsmembers were observed--the 6-month in-program period plus the two years after their exit from Job Corps. However, there is a strong reason to believe that at least some of the benefits discussed will continue to persist after this period. This is particularly true of the earnings gains and of those benefits closely related to increased earnings: increased taxes and reduced transfer payments.

The existence of these future benefits creates a difficult problem for the benefit-cost analysis: in order to provide an accurate assessment, it is necessary to estimate the value of all benefits and costs, and not just those generated during the period covered by the interviews.

While the extrapolations required by a full assessment are important, they are subject to much more uncertainty than the estimates of observed benefits and costs. For these future benefits, not only are there questions about the appropriateness of the valuation methods, but, in addition, the lack of direct observations upon which to base those valuations creates further uncertainty. Future benefits and costs must be estimated on the basis of trends observed to occur during the observation period or on the basis of long-run studies of other groups--both of which are subject to considerable error and serious controversy.

The central hypothesis of the extrapolation procedure is that all benefits observed during a base time period will persist into the future, and that their magnitude will decline continuously over time.[1] Thus, for example, an observed increase in earnings per year during the base period would be assumed to continue for future periods, with the size of the benefit becoming smaller in each succeeding year.

The base period we have adopted is the last 6 months of the observation period. The time horizon over which benefits are assumed to persist is assumed to be, at most, the expected worklife of the average Corpsmember (43 years after enrolling in Job Corps). The rate at which earnings, taxes, and transfer-program benefits are assumed to decay was taken from a study of the long-run effect of Manpower Development and Training Act (MDTA) training programs on participant earnings. This study found that earnings differentials had declined by one-half after five years.[1] While the use of this figure is probably somewhat conservative for Job Corps, it is the most sound estimate available at this date and is roughly consistent with the data available toward the end of the observation period. For all other benefits a much higher rate was used because participation in these activities declines rapidly with age.[2]

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[1] Technical Report K presents a more detailed explanation and justification of the extrapolation procedures and issues covered in this section.

In addition to the extrapolation of effects into the future, two other issues regarding benefits and costs over time had to be addressed. The first was to correct for the effect of inflation on the dollar-denominated benefits and costs. This was done by measuring all items in terms of fiscal year 1977 dollars. This time period corresponds approximately to the period when our sample of Corpsmembers entered Job Corps. As discussed in the next section, cost data were taken from fiscal year 1977 records and require no special adjustment. For the benefits, we used shadow prices measured in fiscal 1977 dollars or adjusted to account for inflation.

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[1] A study by Ashenfelter (1978) provides the best evidence available on the future magnitude of the effect. He found that the earnings gains for adult men who had participated in MDTA employment and training programs had declined by approximately 50 percent after five years, while the gains for adult women did not fade out. If we assume a decline for Job Corps similar to the larger magnitude that Ashenfelter found for males, on a continuous basis it would imply a rate of decline of just under 14 percent per year. In the absence of better information, Ashenfelter's decay rate for adult males has been adopted for both males and females. This probably overstates the decay rates for Corpsmembers, both because Corpsmembers are young and because Ashenfelter's estimated decay rate for males is larger than that for females.

[2] Specifically, it was assumed that these effects decay at a continuous rate that approaches zero by the time Corpsmembers reach an average age of 25 (approximately 5 years after leaving Job Corps). This assumption (a decay rate of 140 percent per year) is consistent with the observation that decreases in arrest rates and drug-treatment use decay very rapidly over the observation period, but is overly conservative regarding the utilization of alternative services.

The second adjustment was to account for the fact that benefits or costs generated in the future will not be worth as much as the same benefits or costs generated at present. This adjustment process (discounting to present values) converts the observed extrapolated streams of benefits and costs into an equivalent dollar amount. We use a real (i.e., net of inflation) discount rate of 5 percent per year. With an inflation rate of 10 percent per year, this would correspond to an annual interest rate of 15 percent.

Table V.8 presents the extrapolated values for the various benefits and costs from the social perspective. The largest effect of extrapolation is for earnings, for which the value of the benefit is increased four times by the addition of extrapolated benefits. The smaller numbers for crime-reduction benefits, drug use, and use of alternative education and training programs reflect (1) the smaller Job Corps effects observed during months 19 to 24 for these benefits and (2) the higher fade-out rate assumed for them. The estimated value of all the measured benefits is approximately $7,000 per Corpsmember (in 1977 dollars).

B. COSTS

The breakdown of program costs by category and analytical perspective is shown in Table V.9. There are three basic cost categories: program operating expenditures; the opportunity cost of Corpsmember labor; and the nonbudgeted costs other than for Corpsmember labor. The total social cost (i.e., excluding all transfers) of Job Corps is estimated to be $5,070 per Corpsmember, while the cost to non-Corpsmembers is $5,736 per Corpsmember enrolled. The difference is the net value of the transfers provided to Corpsmembers ($665 per Corpsmember).

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TABLE V.8

ESTIMATED TOTAL VALUE OF SOCIAL BENEFITS PER CORPSMEMBER[a]

(1977 DOLLARS)

	Discounted Present Value[b]		
	Observation Period	Extrapolation Period	Total Benefits
A. Output produced by Corpsmembers			
• In-program output	$757	$ 0	$ 757
• Increased postprogram output	925	2,971	3,896
• Increased tax payments on postprogram income[c]	0	0	0
• Increased utility due to preferences for work over welfare[c]	+	+	+
B. Reduced Dependence on Transfer Programs			
• Reduced transfer payments[c]	0	0	0
• Reduced administrative costs	63	96	158
C. Reduced Criminal Activity			
• Total reductions in observed resource costs	1,962	150	2,112
• Reduced psychological costs[d]	+	+	+
D. Reduced Drug/Alcohol Use			
• Total reductions in treatment costs	31	-1	30
• Increased utility from reduced drug/alcohol dependence[d]	+	+	+
E. Utilization of Alternative Services			
• Total reductions in costs of training, educational, and PSE programs	85	4	90
• Reduced training allowances[c]	0	0	0
F. Other Benefits			
• Increased utility from redistribution[d]	+	+	+
• Increased utility from improved well-being of Corpsmembers	+	+	+
Total Measured Benefits	$3,823	$3,220	$7,043

NOTE: Details may not sum exactly to totals because of rounding.

[a] See Technical Report K for a detailed treatment from the other perspectives.

[b] Discounted to the in-program period at a 5 percent annual rate (for justification, see the text).

[c] Item does not enter the social perspective (i.e., transfers between Corpsmembers and non-Corpsmembers).

[d] Item is not measured in the analysis.

TABLE V.9

SUMMARY OF ESTIMATED VALUE OF COSTS PER CORPSMEMBER, BY ANALYTICAL PERSPECTIVE[a]

(FISCAL YEAR 1977)

Cost Component	Present Value of Costs by Perspective Social	 Non-Corpsmember	 Corpsmember
A. Program Operating Expenditures			
• Center operating expenditures, excluding transfers to Corpsmembers	$2,796	$2,796	$ 0
• Transfers to Corpsmembers	0	1,208	-1,208[b]
• Central administrative costs	1,347	1,347	0
B. Opportunity Cost of Corpsmember Labor During the Program			
• Foregone output	881	0	881
• Foregone tax payments	0	153	-153[b]
C. Unbudgeted Expenditures Other Than Corpsmember Labor			
• Resource costs	46	46	0
• Transfers to Corpsmembers	0	185	-185[b]
Total Costs	$5,070	$5,736	$ -665[b]

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[a] The cost per Corpsmember is estimated by multiplying the cost per Corpsmember year (as estimated in Technical Report K) by the average length of stay in years for Job Corps during fiscal year 1977--5.9 months.

[b] Because Corpsmembers benefit from transfers, they are presented here as negative costs.

1. Program Operating Expenditures

The breakdown of program operating expenditures into the three components--center operating expenditures (excluding Corpsmember transfers), Corpsmember transfers, and the central administrative costs--reflects the different nature and sources of Job Corps expenditures. Center operating expenditures are costs to non-Corpsmembers and to society. These expenditures and figures were obtained from the Job Corps Financial Reporting System. The Corpsmember transfers were also obtained from the Job Corps Financial Reporting System, but they are not social costs; instead, they represent a transfer of resources from non-Corpsmembers to Corpsmembers.[1] Finally, data on the central administrative expenditures were provided by the Office of Management and Budget. These expenditures represent costs to both non-Corpsmembers and society as a whole.

2. Opportunity Cost of Corpsmember Labor

Youths who participate in Job Corps forego employment opportunties they otherwise would have taken. The wages they would have earned are a cost to them of participating in Job Corps. This "opportunity cost" of Corpsmember labor is not balanced by corresponding benefits to non-Corpsmembers and thus enters into the social benefit-cost calculation as a cost.[2]

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[1] These transfers are expenditures for items that many Corpsmembers would have consumed in the absence of Job Corps (e.g., food, clothing, and housing) and, hence, can be assumed to value near the supply price.

[2] However, if the labor markets are in disequilibrium (i.e., if disadvantaged youths are unemployed in the labor market), non-Corpsmembers receive benefits from replacing Corpsmembers on jobs; thus, social costs are reduced. While replacement is obviously an important factor given the labor markets that Corpsmembers leave when they enter the program, we have no basis currently to estimate either the magnitude or value of replacement activities. Furthermore, we have no means to measure displacement during the postprogram period or to compare its value to that for replacement.

Another way to view this cost is that, from society's point of view, the decision to enroll a person in Job Corps implies that the output that person would have produced in the absence of the program must now be foregone. The loss of this output is a net cost to society; the value of this foregone output is measured by the foregone earnings. As was the case in estimating the increase in output produced, the estimate of foregone earnings includes the amount of fringe benefits in order to measure the total value of the lost output.

3. Nonbudgeted Costs Other than for Corpsmember Labor

The opportunity cost of Corpsmember labor described above is of course an unbudgeted item. In addition, there are other types of expenditures whose costs do not appear in the Job Corps financial accounts. These expenditures include the following items: governmental surplus goods, for which the centers pay only transportation charges; meal costs reimbursed by the National School Lunch program; medical supplies and services provided by state and local agencies; and other resources acquired at below-market prices. The use of these resources is a cost to non-Corpsmembers and to society. However, the use of many of these items represents a transfer to Corpsmembers and, hence, does not enter into the social perspective. The opportunity cost of these resources was estimated on the basis of special studies conducted at thirteen Job Corps centers (see Technical Report F for more details).

C. OVERALL FINDINGS FOR NET PRESENT VALUE

Once the various effects of Job Corps have been valued, calculating the net present value is straightforward. Table V.10 presents the values of the various benefit and cost components with their associated net present values from the three perspectives. As

TABLE V.10

ESTIMATED NET PRESENT VALUES PER CORPSMEMBER UNDER THE BENCHMARK ASSUMPTIONS[a]

(1977 DOLLARS)

	Social	Non-Corpsmember	Corpsmember
BENEFITS			
A. Output Produced by Corpsmembers			
• In-program output	$ 757	$ 673	$ 83
• Increased postprogram output	3,896	0	3,896
• Increased tax payments on postprogram income	0	582	-582
• Increased utility due to preferences for work over welfare	+	+	+
B. Reduced Dependence on Transfer Programs			
• Reduced transfer payments	0	1,357	-1,357
• Reduced administrative costs	158	158	0
C. Reduced Criminal Activity			
• Reduced criminal justice system costs	1,152	1,152	0
• Reduced personal injury and property damage	645	645	0
• Reduced stolen property	315	484	-169
• Reduced psychological costs	+	+	+
D. Reduced Drug/Alcohol Use			
• Reduced treatment costs	30	30	0
• Increased utility from reduced drug/alcohol dependence	+	+	+
E. Utilization of Alternative Services			
• Reduced costs of training, educational, and PSE programs	90	90	0
• Reduced training allowances	0	49	-49
F. Other Benefits			
• Increased utility from redistribution	+	+	+
• Increased utility from improved well-being of Corpsmembers	+	+	+
Total Benefits	$7,043	$5,220	$1,823
COSTS			
A. Program Operating Expenditures			
• Center operating expenditures, excluding transfers to Corpsmembers	$2,796	$2,796	$ 0
• Transfers to Corpsmembers	0	1,208	-1,208
• Central administrative costs	1,347	1,347	0
B. Opportunity Cost of Corpsmember Labor			
• Foregone output	881	0	881
• Foregone tax payments	0	153	-153
C. Unbudgeted Expenditures other than Corpsmember Labor			
• Resource costs	46	46	0
• Transfers to Corpsmembers	0	185	-185
Total Costs	$5,070	$5,736	-665
Net Present Value (Benefits less Costs)	$1,971	-$514	$2,485
Benefit-Cost Ratio	1.39	0.91	1.82

NOTE: Details may not sum exactly to totals because of rounding.

[a] See the text for a review of the assumptions, estimation procedures, and their implications for the values presented in this table.

[b] The numerators for the benefit-cost ratios include all of the benefits listed in this table as either positive benefits or negative costs, and the denominator includes all of the costs listed in this table as either positive costs or negative benefits.

can be seen, the program yields net benefits to society and to Corpsmembers with our benchmark assumptions. From the social perspective, the increase in output and the criminal-justice-system cost savings constitute the bulk of the benefits. The largest social costs are for the resources necessary to operate the program.

Corpsmembers benefit principally from the increase in their postprogram earnings and from the program's transfer (shown as negative costs) in the form of room and board. Their major costs are the foregone earnings while they are in the program, as well as the reductions in the transfer payments that accompany their increases in earnings. Non-Corpsmembers, who bear both the costs of program operation and the costs of the transfers to Corpsmembers, have an estimated net cost for measured Job Corps effects.[1] They do receive substantial benefits from reductions in Corpsmember criminal activity, but these are not sufficient to outweigh their share of program cost. Thus, Job Corps is estimated to be a socially efficient use of resources and to lead to a redistribution of resources from non-Corpsmembers to Corpsmembers.

These benchmark numbers differ from the benchmark reported in the earlier benefit-cost analysis of Job Corps.[2] The major differences are a higher estimate of the increase in postprogram earnings and taxes, and a lower value for the benefits associated with reductions in transfer-program use, criminal activity, drug treatment, and use of alternative education and training programs. These differences are due primarily to the lengthier follow-up data used in this report--data which indicated (1) that earnings gains persisted longer than tentatively assumed in the initial analysis, and (2) that the other effects tended to be smaller and decayed more rapidly than initially estimated.[1]

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[1] Of course, because there are many more non-Corpsmembers than Corpsmembers, the net cost to non-Corpsmembers will, on average, be quite small (much smaller than the $514 per Corpsmember). However, some non-Corpsmembers (e.g., recipients of the value of output and additional victims of crimes in the absence of Job Corps) may benefit substantially.

[2] Mallar et al. (1978) and Technical Report D.

Despite these differences, the major policy conclusion of the two reports is the same: Job Corps is an efficient use of resources. In both cases the social net present value was positive, although the more recent (and more accurate) estimate is substantially larger ($1,971 per Corpsmember compared to the eariler estimate of $251). Thus, this report confirms the overall earlier finding, but with a substantially different benefit composition.

The same is true for the conclusions regarding Corpsmember benefits. The estimate of net present value from this perspective is positive in both studies, with the more recent number being much larger ($2,485 per Corpsmember compared to $212 in the previous study). The reason for this difference is the large upward revision of the increased earnings estimate due to the longer-term observation.

The only major qualitative difference in the reports concerns the effect of Job Corps on non-Corpsmembers. The early evaluation estimated a small positive net present value ($39 per Corpsmember), while this report estimates a negative value (-$514). Thus, the evaluation's earlier conclusion that Job Corps generated positive net benefits from all three perspectives cannot be supported. The findings presented here suggest that the program results in a net transfer of income from non-Corpsmembers to Corpsmembers.

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[1] Technical Report K provides a detailed examination of the differences between the studies.

As mentioned earlier in this chapter there is uncertainty surrounding any single estimate of net present value. Table V.11 presents estimates of the net present value per Corpsmember made under our benchmark and seven alternative sets of assumptions and estimates.[1] In each case, one specific assumption or estimate is changed (with the remaining benchmark assumptions and estimates being maintained).

The first four sets of alternative assumptions in Table V.11 concern the rate at which the Job Corps effects fade out over time. The first alternative presented assumes that there are no effects other than those already observed by the end of the observation period (two years after leaving Job Corps). These estimates indicate that future social benefits (after the first 24 postprogram months) will have to be worth at least $1,249 per Corpsmember if Job Corps is to be considered an economically efficient use of resources.[2] The second alternative assumption is that the earnings and corresponding tax and transfer effects do not fade out, while the other effects (reduced criminal activity, drug- and alcohol-treatment use, and

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[1] Only brief summaries are given here of some of the sensitivity tests made on the various assumptions. More details are presented in Technical Report K.

[2] Note that because the transfers received by Corpsmembers while they are in the program are worth more than their foregone earnings, Corpsmembers will have a positive net present value even if there are no future effects.

TABLE V.11

ALTERNATIVE ESTIMATES OF NET PRESENT VALUE PER CORPSMEMBER[a]

(1977 DOLLARS)

	Analytical Perspective		
Alternative	Social	Non-Corpsmember	Corpsmember
Benchmark assumptions[b]	$1,971 (1.39)	$ -514 (0.91)	$2,485 (1.82)
Effects are zero after the first 24 postprogram months	-1,249 (0.75)	-2,070 (0.64)	821 (1.52)
Earnings and transfer effects do not fade out but other effects do fade out at the benchmark rate	9,384 (2.85)	2,823 (1.49)	6,561 (1.74)
Crime, drug, and alternative program effects fade out at the same rate as earnings effects (14 percent a year)	3,424 (1.68)	1,148 (1.20)	2,276 (2.10)
Crime, drug, and alternative program effects are zero after the first 24 postprogram months	1,817 (1.36)	-690 (0.89)	2,507 (1.88)
3 percent discount rate	2,546 (1.50)	-234 (0.96)	2,780 (1.89)
10 percent discount rate	940 (1.19)	-1,028 (0.82)	1,968 (1.80)
No underreporting of arrests	1,101 (1.22)	-1,453 (0.75)	2,555 (1.93)

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[a] The numbers in parentheses below the estimates of the net present value are benefit-cost ratios computed as described in Table V.10.

[b] Benchmark assumptions are as follows: earnings, tax, and transfer effects fade out at a rate equal to 50 percent every five years; all other effects fade out completely five years after leaving Job Corps; the discount rate is 5 percent; the expected worklife of a Corpsmember is forty-three years after leaving Job Corps. (For a more complete discussion of these and other assumptions, see Chapter V of Technical Report D.)

education- and training-program use) fade out at the benchmark rate.[1] In this instance, the social, Corpsmember, and non-Corpsmember net present values are all positive, with the total social benefits outweighing social costs by almost $9,400 per Corpsmember.

The third and fourth alternatives illustrate the effect of changing the rate at which the reduced crime, drug treatment, and alternative education- and training-program use effects fade out. If these effects fade out at the lower rate (14 percent per year) assumed for earnings and transfers, estimated net present value would be greater than with the benchmark assumptions, and would be positive from all three perspectives. The social net present value would be almost doubled--to $3,424 per Corpsmember. On the other hand, if these effects do not persist after the two-year postprogram observation period, then net present value will be lowered. This decline ($154 per Corpsmember) is not large, however, because the future value of the estimated effects is small under the benchmark assumptions of an extremely rapid fade-out rate (140 percent a year) and short-time horizon (5 years after the observation period ends).[2]

The appropriate discount rate to use when evaluating government training and educational programs is always a controversial issue because, while the choice of a discount rate is very important for the evaluation and is well established theoretically, there has never been a completely satisfactory way to estimate discount rates. Imperfections in the markets for capital, the existence of risk and uncertainty, inflation, and the fact that many tax-incidence questions are still unresolved have made it impossible to determine a single discount rate appropriate for evaluating goverment investments. As a result, we have adopted a 5 percent real rate (i.e., net of inflation) as our benchmark.[1/] Because of the somewhat arbitrary nature of this assumption, we test the sensitivity of the findings to variations in this assumption.

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[1] This test was adopted because there is evidence that earnings and transfer effects are not fading out, but that criminal activity, drug use, and participation in training and educational programs decline as people grow older. As a result, the magnitudes of reductions in these latter activities would probably fade out even if the percentage reduction due to Job Corps participation did not.

[2] For example, one year after the end of the observation period the value of these effects would be reduced 77 percent under the benchmark fade-out assumptions. After two years, the value would be reduced by 95 percent.

To test the sensitivity of the findings to assumptions about the discount rate, net present value estimates were made by using 3 and 10 percent real discount rates. (These alternative estimates are presented in Table V.11). As can be seen, the social net present value changes in the opposite direction from the discount rate. Lower discount rates increase the present value of social benefits, but leave social costs unchanged because all of them are incurred during the initial time period. Thus, using a 3 percent discount rate increases the social net present value by approximately $575 per Corpsmember compared to the benchmark estimate. In a similar manner, increasing the discount rate from the 5 percent benchmark rate to 10 percent decreases the social net present value by over $1,000 per Corpsmember, although it is positive.

The last sensitivity test presented in Table V.11 concerns the estimation of the crime-reduction benefits. The benchmark procedure adjusts the arrest data to account for the underreporting of arrests

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[1/] For further documentation, see Technical Report K.

in the interviews. However, the estimate of underreporting is subject to some uncertainty and may not be entirely appropriate for the sample of youths enrolled in Job Corps.[1] To see the effect of this adjustment, we have estimated net present value using the unadjusted self-report data on arrests. This has the effect of dividing the benchmark estimates of the crime reduction benefits by 1.7. The resulting net present values display the same pattern as the benchmark values, but are almost 50 percent smaller from the social perspective (net present value from the Corpsmember perspective rises because the estimate of foregone theft income is reduced).[2]

The general conclusion of these sensitivity tests is that as long as the earnings gains do not decay extremely rapidly (e.g., greater than 37 percent per year) after the two-year postprogram observation period (and there is no evidence that they will), Job Corps is an economically efficient use of resources.[3] In addition, Corpsmembers receive large positive net benefits, while non-Corpsmembers as a group will bear some costs of a redistribution of income. Under diverse assumptions, the sensitivity tests indicate that Corpsmembers will receive a substantial net benefit, probably worth approximately $2,500 on average. Finally, if the various benefits that are left unmeasured (preferences for having Corpsmembers lead "more acceptable" life-styles; reduced psychological costs of crime; and satisfaction from the redistribution of income and improved well-being to Corpsmembers) could be added to the measured benefits, the results would undoubtedly be even more favorable toward Job Corps, especially as far as non-Corpsmembers are concerned.

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[1] The underreporting estimate was made by examining official records and interview data that used questions and survey techniques similar to those used in the Job Corps interviews. However, the interview data used in the validation study were for a sample of slightly older ex-offenders and ex-addicts. Thus, while the results are the best available for our purposes, they have a large potential for error.

[2] Other assumptions are examined in more detail in Technical Report K and are found not to substantially alter the results.

[3] If only earnings and transfer effects persist into the future, then at a 5 percent discount rate social net present value will be positive as long as these effect do not fade out more rapidly than 37 percent per year. If all effects persist and decay at the same rate, social net present value will be positive if the rate is less than 57 percent per year. Alternatively, if all effects persist unchanged for a year and a half past the end of the observation period, social net present value will be positive.

D. SUMMARY AND CONCLUSIONS

The principal issue analyzed in this chapter is whether the investment in Job Corps is economically efficient--specifically, does society have more goods and services at its disposal because of the investment in Job Corps? The findings of this analysis suggest that public investment in Job Corps is efficient. Our benchmark estimate is that the social value of benefits in fiscal year 1977 exceeds costs by almost $2,000 per Corpsmember, or by approximately 39 percent of costs, and the program is found to be efficient under a wide range of alternative assumptions and estimates. Because over 40,000 youths enrolled in Job Corps during fiscal year 1977, our benchmark estimate of the net social benefit is approximately $79 million for that year.

We estimate that over 60 percent of the social benefits are generated by increases in the value of output that Corpsmembers produce. Another 30 percent of the social benefits are attributable to reductions in criminal activity among Corpsmembers--particularly burglary and larceny. These benefits from reductions in crime include reductions in personal injury, property damage, stolen property, and criminal justice system costs. The bulk of the social costs are incurred from operating the program.

The analysis of social benefits and costs abstracts from the fact that members of society share disproportionately in the benefits and costs. The equity effects of the program are important for a complete analysis of the program. As a result, we also analyzed the benefits and costs of investments in Job Corps from the perspectives of both Corpsmembers and all other members of society (non-Corpsmembers). Our benchmark distributional estimates indicate that the average Corpsmember receives a net benefit of $2,485 from participating in Job Corps. We estimate that non-Corpsmembers as a group incur a net cost of $514 per Corpsmember.

Approximately 70 percent of the benefits to Corpsmember are accounted for by their increased earnings. The other major benefits are the transfers they receive while they are in Job Corps. The largest cost borne by Corpsmembers is the reduction in their transfer income, although the opportunity cost of the time they spend in Job Corps and the increase in tax payments are also significant costs for them.

Non-Corpsmembers receive over $5,200 per Corpsmember in benefits, mostly from the reductions in Corpsmembers' criminal activity and the use of transfer programs. However, they incur almost all of the costs for the operation and administration of the program. As a result, measured costs exceed measured benefits from the non-Corpsmember perspective of the Job Corps expenditures, including a large transfer to Corpsmembers.

Estimating the present value of benefits and costs required numerous assumptions and approximations. In particular, because this analysis is based on interview data that covered, on average, only two years of postprogram activity, we have had to make some speculative assumptions about the magnitude of Job Corps effects after this observation period. We assume that the Job Corps effects on earnings, tax payments, and transfer-program use fade out at approximately 14 percent per year. The other effects--reductions in crime, in drug and alcohol treatment, and in the use of alternative education and training programs--were assumed to fade out entirely 5 years after leaving Job Corps (an extremely fast fade-out rate). Sensitivity tests of this and other assumptions suggest that as long as earnings effects do not decay more rapidly than 37 (32) percent per year, assuming a real discount rate of 5 (10) percent a year, we estimate that Job Corps is an efficient use of resources.[1/] Alternatively, all observed program effects would have to be maintained for approximately one more year in order for the social net present value to be positive.

[1/] Note that our benchmark assumptions include an even higher decay rate for other effects, so that to the extent that the observed beneficial effect are maintained or grow, the implicit internal rate of return is very large.

VI. DIFFERENTIAL IMPACTS AMONG CORSPMEMBERS AND CENTERS

The discussion of the estimates of overall Job Corps impacts on employment and related activities in Chapter III and its companion discussion in Chapter IV provide only a limited indication of differential impacts among Corpsmembers and centers. One set of differentials noted in these earlier chapters is the differential between males and females, and, for females, between those without and those with children. As explained in Chapter III, our previous research (see Mallar et al., 1978) found that the behavioral relationships of interest were substantially different for these three subgroups, based on statistical tests for differences in parameters (Chow tests). With an appropriate specification, however, we found that observations of youths could be pooled together across other demographic classifications.

We begin this chapter with a review of the pattern of differences in impacts between the sexes, including differences by presence of dependent children for females. We then discuss differential impacts by Job Corps completion category, which were introduced but not fully explored in Chapter III. We conclude this chapter by examining differential impacts by program treatments among Corpsmembers and centers, which is clearly a most important dimension for considering improvements to program operations. However, for our exploratory analysis of the differential impacts by program treatments, it is not possible to separate the causality of differences in program treatments from underlying differences in the subgroups receiving those program treatments (see further below).

A. DIFFERENTIALS BY SEX AND FAMILY RESPONSIBILITY

The Job Corps impacts are quite different in magnitude for males and females, and among females they are different for those without and those with children. These impacts are detailed in Tables III.2 through III.6 and in Table IV.4 of their respective chapters. Representative impacts for the 12- to 18-month period after termination are summarized in Table VI.1.

An inspection of the estimated effects presented in Table VI.1 finds that Job Corps impacts for males are larger relative to females without children for the receipt of Unemployment Insurance and for the probability of being in military service. While the impacts on Unemployment Insurance are also important for females without children, the larger impacts for them relative to males are for employment and earnings, receipt of welfare, and education. However, these comparisons require some qualification because the base levels of activity are different for the two groups--that is, males and females without children behave differently in the absence of Job Corps participation. For example, females have a lower probability of employment and earnings in the absence of Job Corps participation. Therefore, the estimated differences in impacts by sex are even larger in percentage terms for employment and earnings. The opposite appears to be the case for the receipt of welfare and education measures, for which larger base levels of activity for females in both cases suggests smaller percentages compared to absolute differentials between males and females.

The impacts estimated for females with children are generally quite different from those estimated for the other two groups. Employment and earnings impacts, for example, are small in magnitude and are not significantly different from zero. However, they are sensitive

TABLE VI.1

REPRESENTATIVE ESTIMATES OF DIFFERENTIAL IMPACTS BY SEX AND FAMILY RESPONSIBILITIES 12 TO 18 MONTHS AFTER TERMINATION[a]

Variable	Job Corps Effects for Males	Job Corp Effects for Females: Without Children	Job Corp Effects for Females: With Children[b]
Employed (fraction of time)	0.079*	0.099**	-0.009
Earnings per Week (dollars)	8.63	9.69*	-0.82
Receipt of AFDC or General Assistance (fraction of time)	-0.030***	-0.138****	-0.046*
Receipt of Unemployment Insurance (fraction of time)	-0.025****	-0.019***	not estimated
Probability in Military during Survey Week	0.056***	0.0003	0.000
Probability of having a High School Diploma or GED by Time of Interview	0.187****	0.533****	not estimated

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] Most of the variables in this table are estimated for the civilian population only. The one exception is for the probability of being in the military during the interview week.

[b] Estimates are obtained from models which exclude fertility variables (see Chapter IV); including the fertility variable makes the estimates much more favorable for Job Corps. Also, the estimated impacts for early dropouts must be assumed to equal zero because of the extremely small sample sizes.

to the inclusion of fertility characteristics in the estimating model--that is, they are much more positive when fertility characteristics are included (see Chapter IV). The other major trends are reductions in the use of public assistance (primarily AFDC) and, perhaps surprisingly, increases in the fraction of time spent in training programs (not shown in Table VI.1; see Chapter IV). The impact on public assistance for females with children is much larger than that for males and childless females in absolute magnitude, but smaller in percentage terms.

B. DIFFERENTIALS BY COMPLETION CATEGORIES

Differences in impacts by completion categories are important in this evaluation for two reasons. First, they are inherently interesting for program operations as measures of the effects of varying the length of stay in Job Corps or the degree of program completion.[1] Second, we need to estimate the impacts for each completion category so that we can reweight them in order to obtain overall impact estimates that are indicative of the average for all Job Corps enrollees (see Chapter III for more details on this procedure).

An important issue in drawing inferences for program operations is whether we have obtained generally unbiased estimates of effects by completion category (this does not affect the unbiasedness of the overall estimates). As we described in Chapter III, we have not been able to obtain reliable estimates that control for unobserved differences among Corpsmembers by completion category. We argued that the effects are plausibly unbiased, however, for a number of reasons. First, the pattern of effects by completion category is reasonable; in particular, the effects estimated for the group with near zero treatment (early dropouts) are close to zero. Second, we control for a wide range of observable variables, some of which may also be proxy controls for the effects of unobservable characteristics. Third, any remaining causes of bias are likely to be in opposite directions and at least partially offsetting; for example, the Job Corps completion category includes both youths who are highly motivated and able (i.e., high benefits to staying in the program) and youths who have little initiative and poor labor-market opportunities (i.e., low opportunity costs to staying in the program). The rest of this section will provide additional evidence on the first of these points--the pattern of effects by completion categories.

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[1] As explained in Chapter III, completion category is not perfectly correlated with length of stay because of the individualized and self-paced nature of Job Corps instruction.

Patterns of effects over time for each completion category are shown in Figures III.1 through III.10 in Chapter III. The differentials among the three categories are exactly what we might expect--there is a strong correlation between the amount of the program completed and Job Corps impacts. Program completers fairly consistently benefit the most from their Job Corps experience; partial completers benefit somewhat less; and early dropouts benefit little or not at all. (The sample sizes for early dropouts are relatively small, and the estimated impacts fluctuate a good deal over time. However, they tend to fluctuate around zero.) Among program completers, there are large and relatively uniform effects (as evidenced by relatively large estimated effects and relatively small standard errors).

Job Corps impacts by completion categories for a specific time period (12 to 18 months after termination) are presented in Table VI.2. The differential impacts by category generally follow the pattern described above, with only a few exceptions. The most notable exception

TABLE VI.2

REPRESENTATIVE ESTIMATES OF DIFFERENTIAL IMPACTS BY COMPLETION CATEGORIES 12 TO 18 MONTHS AFTER TERMINATION[a]

Variable	Males			Females Without Children		
	Program Completers	Partial Completers	Early Dropouts	Program Completers	Partial Completers	Early Dropouts
Employed (fraction of time)	0.155****	0.044*	0.047	0.176****	0.081*	0.054
Earnings per Week (dollars)	24.79****	-1.25	3.92	30.36****	14.63**	5.92
Receipt of AFDC or General Assistance (fraction of time)	-0.044****	-0.033***	-0.018	-0.170****	-0.145****	-0.108****
Receipt of Unemployment Insurance (fraction of time)	-0.016**	-0.030****	-0.027****	-0.012*	-0.022****	-0.022**
Probability in Military during Survey Week	0.087****	0.058***	0.032	0.011	-0.003	-0.005
Probability of having a High School Diploma or GED by Time of Interview	0.399****	0.143****	0.049*	0.683****	0.392****	0.525****

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

NOTES: The significance levels given here may be slightly biased because the estimates of standard errors used for the underlying significance tests were obtained from a regression program which does not account for the implicit heteroscedasticity when controlling for unobserved differences between Corpsmembers and the comparison sample via the Heckman (1979) approach. In practice, however, the significance levels from the regression program are usually very close to those from test statistics using unbiased estimates of standard errors, especially when the coefficients for the adjustment variables are statistically insignificant (which is usually the case here). Therefore, the significance levels given here are approximately accurate and are indicative of the true significance levels.

[a] Most of the variables in this table are estimated for the civilian population only. The one exception is for the probability of being in the military during the interview week.

is the pattern for the receipt of Unemployment Insurance. However, the lower reductions in the level of receipt for program completers may be due to the higher level of employment and, hence, qualification for Unemployment Insurance benefits for this group. The pattern is also somewhat unclear for education and the receipt of welfare among females. Overall, these results lend support to the conclusion that the observed differences by completion category are at least partially attributable to a program effect.

Additional evidence for differentials by completion categories comes from Corpsmembers' self-evaluation of the impact of Job Corps participation on future employment. Specifically, for each job Corpsmembers had they were asked whether the training, work experience, or education they received in Job Corps helped them obtain that job.[1] These responses were tabulated to show the proportion who reported their Job Corps program helped them obtain a job, as well as the proportion of jobs in the postprogram period that were obtained with help from the Job Corps program. As summarized in Table VI.3, there is a strong correlation between program completion and the amount of help from their program treatment. Almost two-thirds of the program completers reported that their Job Corps program helped them obtain at least one job. The proportions for partial completers and early dropouts are one-third and one-quarter, respectively. The differentials are even greater when all jobs in the postprogram period are considered: program completers reported that their Job Corps program helped them obtain 45 percent of all jobs held in the period; the figure is only 20 percent for partial completers and less than 10 percent for early dropouts.

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[1] Education was not specifically included in the wording for the First Follow-Up Questionnaire, which undoubtedly leads to some unknown amount of downward bias.

TABLE VI.3

CORPSMEMBERS' SELF-EVALUATION OF JOB CORPS IMPACTS ON EMPLOYMENT

	All Corpsmembers	Program Completers	Partial Completers	Early Dropouts
Proportion who reported that Job Corps training, work experience, or education was helpful in obtaining at least one job[a]	.502	.626	.347	.261
Proportion of all jobs in the post-program period for which Corpsmembers' program was of help in obtaining	.346	.450	.240	.095

[a] Education was not specifically included in the wording for the First Follow-Up Questionnaire, which undoubtedly leads to some unknown amount of downward bias.

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The larger beneficial impacts observed for program completers does seem at least partially attributable to the effect of staying in the program longer and completing. Therefore, the overall performance of Corpsmembers would likely be improved if they could be induced to stay in the program longer so that more of them could complete their Job Corps education and training.

C. DIFFERENTIALS BY CORPSMEMBER CHARACTERISTICS AND PROGRAM TREATMENTS

We have previously reported (Mallar et al., 1978) the results of our first attempt at analyzing differences in impacts among Corpsmembers and centers other than those associated with sex and completion category. Corpsmember characteristics were measured by interview data, while general center characteristics were taken from published sources (no MIS data on Corpsmembers had been linked to our sample members' interview records at that time). The analysis focused on labor-market impacts (as measured by employment and earnings) and Job Corps completion and length of stay.

No large or statistically significant differences in the employment and earnings impacts of Job Corps participation were correlated with the center characteristics, which included administration, operator, size, location, or co-educational status. However, characteristics of Corpsmembers did seem to affect the subsequent labor-market performance of participants. For females without children, those who had a high school diploma at preenrollment had higher levels of employment and earnings in the postprogram period. In addition, Corpswomen who were at least 18 years of age when they left Job Corps tended to have higher earnings. While race/ethnicity differences appear, they are not significantly different from the overall means for both employment and earnings. Among Corpsmen, both race/ethnicity and age at termination influenced their postprogram labor-market experiences. Hispanics and whites tended to have much higher levels of employment and earnings than did either blacks or American Indians. In addition, those who were at least

18 years of age when they terminated from the program tended to have better postprogram labor-force experiences by both measures.

With the availability of the MIS data base and its detailed Corpsmember and center-related information on program treatments, we sought to explore further the differential impacts among Corpsmembers and centers with the last full quarter (approximately 12 to 15 months, on average, after leaving Job Corps)--eighth-quarter measures of employment and earnings. We included in the analysis the same Corpsmember characteristics which showed differentials in the earlier analysis, with one change: the specification of age was changed from age at termination to a more policy-relevant measure, age at enrollment. In place of the general center characteristics which showed no differential impacts, we included in the analysis (1) the occupational cluster associated with Job Corps training that each Corpsmember received, (2) whether or not each Corpsmember completed the Job Corps GED program, and (3) some more refined center variables (see further below).

The estimates of differential impacts associated with program treatments that are presented in the remainder of this chapter should be regarded as only exploratory because of selection and unobserved variable problems which preclude drawing inferences about the causality of observed correlations. Corpsmembers are not randomly assigned to program treatment; rather, Job Corps explicitly attempts to adapt its program to fit the specific needs of each individual youth. Therefore, the differential impacts that we observe for program treatments may be caused by underlying differences in ability, motivation, or other socioeconomic background factors that affect program assignments but that are not observed in our evaluation data. For example, new Corpsmembers who cannot read at all are often initially assigned to a vocational training program that does not require any reading and thus appears to be relatively ineffective (even if it is in fact very effective), because the Corpsmembers who are assigned to it have lower ability and less preparation than average, and we do not control for nonrandom assignments based on reading level at entry.

As shown in Tables VI.4 and VI.5, subsequent employment and earnings performance is associated with race/ethnicity and high school completion prior to entering Job Corps. (All of the effects in this chapter are estimated with regression models that control for all other characteristics noted, as well as for pre-enrollment work histories.) Differences for race/ethnicity are consistently significant for males, and significant in the case of employment for females. During the postprogram observation period, whites and Hispanics seem to have higher levels of employment and earnings than blacks and American Indians. Except, perhaps, for employment among males, receiving a high school diploma prior to entering Job Corps is associated with higher levels of employment and earnings. Age at enrollment appears to make little difference for subsequent labor-market outcomes.

Of the two center-related variables, it is reasonable to expect that the differential effect of completing a GED program in Job Corps is approximately the same as the differential effect of receiving a high school diploma prior to entering Job Corps. That expectation is generally confirmed, particularly for earnings. For males, the type of occupational training received affects the Job Corps impacts. Ninety percent of the Corpsmen were in one of five clusters--Service Occupations, Food Service, Automotive and Machinery Repair, Construction Trades, and Industrial Production. Corpsmen who were in the Service and Food Service training programs are subsequently employed a lower than average percent of the time, while those in Automotive and Machinery Repair and Industrial

TABLE VI.4

DIFFERENTIAL IMPACTS AMONG CORPSMEMBERS AND PROGRAM TREATMENTS FOR MALES DURING THE LAST FULL QUARTER OF OBSERVATION[a]

Subgroup	Employment (Overall Mean: 0.64)	Earnings (Overall Mean: 106.36)	Length of Stay (Overall Mean: 295.54)	Program Completion (Overall Mean: 0.54)
Corpsmembers				
Race/ethnicity				
• Black (n = 577)	0.63	94.90	320.97	0.56
• White (n = 197)	0.69	141.63	228.56	0.48
• Hispanic (n = 89)	0.68	112.25	309.29	0.64
• American Indian (n = 31)	0.46	74.14	225.41	0.34
• Other Race/Ethnicity (n = 10)	0.61	109.96	235.22	0.40
(F-value)	(2.611)***	(8.908)****	(14.279)****	(3.608)****
Age at enrollment				
• 15 (n = 2)	0.48	118.52	161.61	0.03
• 16 (n = 216)	0.64	98.80	299.42	0.51
• 17 (n = 239)	0.63	106.15	295.17	0.50
• 18 (n = 168)	0.66	113.62	307.38	0.61
• 19 (n = 140)	0.63	106.36	267.04	0.53
• 20 (n = 80)	0.67	112.48	301.25	0.58
• 21 (n = 57)	0.61	109.10	315.19	0.63
• 22 (n = 2)	0.42	-39.70	275.26	0.59
(F-value)	(0.292)	(0.804)	(1.241)	(1.569)*
High school diploma prior to entering Job Corps				
• No (n = 792)	0.63	102.91	293.76	0.51
• Yes (n = 112)	0.68	128.12	308.24	0.76
(F-value)	(1.093)	(5.307)***	(0.716)	(23.090)****
Center				
Completion of GED program in Job Corps				
• No (n = 751)	0.63	103.73	279.57	0.47
• Yes (n = 153)	0.68	118.40	373.82	0.88
(F-value)	(1.172)	(2.508)*	(44.199)****	(85.691)****
Occupational Training Cluster				
• Sub professional (n = 11)	0.55	98.63	316.21	0.49
• Clerical and sales (n = 20)	0.63	129.51	229.02	0.29
• Service occupations (n = 93)	0.58	99.78	282.55	0.55
• Forestry, farming and gardening (n = 15)	0.67	72.41	278.30	0.68
• Food service (n = 84)	0.58	88.81	272.79	0.63
• Automotive and machinery (n = 140)	0.70	108.27	333.62	0.55
• Construction trades (n = 359)	0.62	105.49	294.91	0.53
• Electrical/appliance repair (n = 18)	0.68	94.90	292.35	0.52
• Industrial production (n = 140)	0.69	121.05	307.48	0.59
• Transportation (n = 7)	0.98	202.89	200.70	0.11
• Health occupation (n = 7)	0.80	83.43	374.93	0.46
• Unassigned (n = 10)	0.57	84.33	134.13	0.13
(F-value)	(1.351)*	(1.702)**	(3.024)****	(2.291)****

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for one-tail test).

FOOTNOTE FOR TABLE VI.4

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[a] The findings presented in this table are based on multiple regressions; therefore, the estimates for each subgroup are net of the independent influences of all other subgroups in the table (i.e., we control for all of the variables in the table simultaneously). The estimates for all relevant subgroups would have to be averaged in order to obtain the estimate for a particular set of Corpsmember and center characteristics. The F-statistics shown in parentheses at the bottom of each factor (subset) are for null hypotheses that all of the regression coefficients underlying the results shown for each factor are simultaneously equal to zero.

TABLE VI.5

DIFFERENTIAL IMPACTS AMONG CORPSMEMBERS AND PROGRAM TREATMENTS FOR FEMALES WITHOUT CHILDREN DURING THE LAST FULL QUARTER OF OBSERVATION[a/]

Subgroup	Employment (Overall Mean: 0.53)	Earnings (Overall Mean: 69.68)	Length of Stay (Overall Mean: 298.34)	Program Completion (Overall Mean: 0.67)
Corpsmembers				
Race/ethnicity				
• Black (n = 153)	0.48	64.71	309.19	0.72
• White (n = 65)	0.54	74.10	220.09	0.49
• Hispanic (n = 40)	0.70	80.55	405.57	0.80
• American Indian (n = 7)	0.52	57.13	210.55	0.61
• Other Race/Ethnicity (n = 3)	0.98	112.88	188.88	0.39
(F-value)	(2.287)**	(0.791)	(9.267)****	(4.101)****
Age at enrollment				
• 16 (n = 39)	0.47	65.87	308.54	0.74
• 17 (n = 57)	0.43	60.21	289.48	0.70
• 18 (n = 62)	0.63	85.58	296.37	0.69
• 19 (n = 48)	0.53	55.50	302.80	0.56
• 20 (n = 40)	0.53	71.99	291.65	0.62
• 21 (n = 21)	0.57	80.29	324.35	0.74
• 22 (n = 1)	0.93	120.90	27.16	0.81
(F-value)	(1.275)	(1.155)	(0.685)	(0.720)
High school diploma prior to entering Job Corps				
• No (n = 180	0.47	58.64	291.94	0.56
• Yes (n = 88)	0.66	91.05	311.58	0.90
(F-value)	(6.708)****	(6.635)***	(0.602)	(22.174)****
Center				
Completion of GED program in Job Corps				
• No (n = 215)	0.51	66.23	274.87	0.60
• Yes (n = 53)	0.60	85.29	393.09	0.96
(F-value)	(1.174)	(1.667)*	(17.043)****	(20.503)****
Occupational Training Cluster				
• Sub professional (n = 2)	0.51	-0.58	274.16	0.54
• Clerical and sales (n = 130)	0.61	80.53	306.97	0.61
• Service occupations (n = 4)	0.53	53.66	424.02	1.05
• Forestry, farming and gardening (n = 1)	0.56	66.29	530.77	1.08
• Food service (n = 20)	0.54	56.15	304.65	0.87
• Automotive and machinery (n = 1)	0.79	85.78	330.30	0.79
• Construction trades (n = 14)	0.50	85.85	274.85	0.47
• Electrical/appliance repair (n = 2)	-0.05	--	376.54	0.72
• Industrial production (n = 13)	0.30	49.28	342.56	0.86
• Transportation (n = 5)	0.50	49.96	374.61	1.00
• Health occupation (n = 74)	0.47	60.67	265.86	0.68
• Unassigned (n = 2)	-0.11	-11.54	128.34	0.58
(F-value)	(1.449)*	(1.158)	(1.287)	(1.692)**

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for one-tail test).

FOOTNOTE FOR TABLE VI.5

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[a] The findings presented in this table are based on multiple regressions; therefore, the estimates for each subgroup are net of the independent influences of all other subgroups in the table (i.e., we control for all of the variables in the table simultaneously). The estimates for all relevant subgroups would have to be averaged in order to obtain the estimate for a particular set of Corpsmember and center characteristics. The F-statistics shown in parentheses at the bottom of each factor (subset) are for null hypotheses that all of the regression coefficients underlying the results shown for each factor are simultaneously equal to zero.

production have the opposite experience. This pattern of differential impacts generally carries over to earnings.

For females, the type of occupational training received affects only the Job Corps impacts on employment. Of the clusters with the largest enrollment, Corpswomen who were enrolled in Clerical and Sales are employed a greater than average proportion of the time, while those who were enrolled in Construction Trades, Health Occupations, and particularly Industrial Production are employed a lower than average proportion of the time. While most of this pattern for females carries over to earnings, the differential cluster effects are not statistically significant.

Since completion of the Job Corps program and length of stay seem to positively affect the various measures of economic performance, the differential impacts on these two factors are also important.[1/] Blacks and Hispanics clearly are more likely than whites and American Indians to stay longer in Job Corps and also to complete a program. Receiving a high school diploma prior to entering Job Corps increases the likelihood of completing a program, but does not significantly affect length of stay. Once again, age at enrollment has no effect.

Completing a GED program in Job Corps would seem to require a Corpsmember to stay in the program longer, and that pattern is quite evident. A strong positive correlation also exists between completing a GED program and being classified as a Job Corps completer.

[1/] Regressions with months in Job Corps as the treatment measure, instead of the three program completion categories, show positive and statistically significant effects on postprogram employment and earnings associated with an additional month in the program.

For females, the type of training affects only completion, and in a pattern that does not closely follow the differential impacts on employment. Corpswomen who were enrolled in Clerical and Sales and Construction Trades training programs are less likely than average to be completers, while those who were enrolled in Food Service and Industrial Production training programs are more likely. The pattern that emerges from this analysis and our earlier analysis of center-related variables is that, while distinctions in general center characteristics do not show differential impacts, distinctions in the actual training and education treatments Corpsmembers receive at centers do suggest differential impacts.

Another view concerning differential impacts that is often put forth by researchers and program personnel is that environment is an important determinant of how much an individual Corpsmember gets out of the program. In this view, important determinants of Job Corps impacts are (1) how center characteristics relate to a Corpsmember's home environment and (2) how his or her activities in Job Corps relate to what others are doing at the center.

As a test of this view, we analyzed the relationship between the outcome measures used in the previous two tables and several new measures of center characteristics that were constructed partially with MIS data. The first set of variables indicates the similarity between Corpsmembers and the centers they attended for sex, race/ethnicity, and location (i.e., rural-urban). This is measured as a mean absolute deviation between a Corpsmember's characteristics and his or her center's characteristics.[1]

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[1] The mean absolute deviation is formally defined as $\frac{1}{n}\left|\sum_{i=1}^{n} X_{ai} - X_{di}\right|$ where n is the number of cells, X_{ai} is the actual value of X for the ith observation, and X_{di} is some designated level of X for that observation. If, for example, we are measuring sex and the observation is

Thus, a smaller value for this variable (i.e., approaching zero) indicates less deviation and greater similarity; a larger value (i.e., approaching one) indicates more deviation and less similarity. The second set of variables indicates the similarity between the characteristics of the center attended and a uniform distribution of characteristics (i.e., 50 percent male and 50 percent female for sex; 25 percent each of black, white, Hispanic, and American Indian for race/ethnicity; and 50 percent urban and 50 percent rural for location). Once again, the similarity is measured as mean absolute deviations, so that lower calculated values indicate less deviation and greater similarity to a uniform distribution. Finally, we include a variable which measures directly the percentage of Corpsmembers in the center attended who are enrolled in a GED program, and a set of variables which measure the percentage who are enrolled in each occupational training cluster. All of the effects were estimated with regression models which also control for race/ethnicity, education, age, and baseline work histories.

The results for males and females are shown in Tables VI.6 and VI.7, respectively. The variables indicating similarity between Corpsmembers and their centers do not strongly support the importance of such similarity. The only cases in which effects are statistically significant are the sex and location effects on length of stay for males and the sex effect on length of stay for females. As was expected, all of these effects are negative--that is, greater similarity in sex or location is associated with increased length of stay at centers.

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female, but the center is only 40 percent female ($X_d = .4$), the mean absolute deviation equals .60. If, on the other hand, the center is 80 percent female ($X_d = .8$), the mean absolute deviation equals .20. Finally, if the center is all female ($X_d = 1.0$), the mean absolute deviation equals zero.

TABLE VI.6

DIFFERENTIAL IMPACTS BY CENTER CHARACTERISTICS FOR MALES DURING THE LAST FULL QUARTER OF OBSERVATION[a]

Variable	Employment	Earnings	Length of Stay	Program Completion
Mean absolute deviation between Corpsmembers and Center characteristics[b]				
• Sex	-0.431	-26.728	-11.839****	-0.286
• Race/ethnicity	0.007	-33.652	0.143	-0.144
• Location	0.088	14.618	-2.091***	0.047
Mean absolute deviation between Center and uniform distribution of characteristics				
• Sex	-0.595***	-78.744*	-11.199****	-0.809****
• Race/ethnicity	-0.195	-110.58**	3.474	-0.085
• Location	-0.225	15.081	0.724	-0.250
Percent of Corpsmembers in centers enrolled in GED program	.045	38.632*	-0.020	-0.058
Percent of Corpsmembers in centers enrolled in Occupational Training Cluster[c]				
• Sub professional	0.800	-349.67*	-16.976	1.280
• Clerical and sales	0.510*	51.374	3.864	-0.062
• Service occupations	0.080	-65.577*	-2.010	0.006
• Forestry, farming and gardening	-0.435	-451.02*	-0.934	0.316
• Food service	-0.316	-92.168	-6.065	0.762**
• Automotive and machinery repair	0.037	0.212	3.203*	-0.085
• Electrical/appliance repair	0.090	127.49	-8.474	-2.866**
• Industrial production	-0.038	26.872	-0.044	0.184
• Transportation	0.661	181.69	2.995	-1.899**
• Health occupations	0.169	33.111	19.308****	-0.458
• Unassigned	0.040	207.64	6.279	0.939
• (F-value for all training variables)	(.593)	(1.760)**	(1.918)***	(1.429)*

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

[a] The findings presented in this table are based on multiple regressions; therefore, the estimates for each subgroup are net of the independent influences of all other subgroups in the table (i.e., we control for all of the variables in the table simultaneously). The estimates for all relevant subgroups would have to be averaged in order to obtain the estimate for a particular set of Corpsmember and center characteristics. The F-statistics shown in parentheses at the bottom of each factor (subset) are for null hypotheses that all of the regression coefficients underlying the results shown for each factor are simultaneously equal to zero.

[b] The mean absolute deviation is formally defined as $\frac{1}{n}\left|\sum_{i=1}^{n} X_{ai} - X_{di}\right|$, where n is the number of cells, X_{ai} is the actual value of X for the ith observation, and X_{di} is some designated level of X for that observation.

[c] The Construction Trades occupational cluster is the omitted category. The differential impacts estimated for all other clusters are deviations from the impact for the Construction Trades cluster.

TABLE VI.7

DIFFERENTIAL IMPACTS BY CENTER CHARACTERISTICS FOR FEMALES WITHOUT CHILDREN DURING THE LAST FULL QUARTER OF OBSERVATION[b]

Variable	Employment	Earnings	Length of Stay	Program Completion
Mean absolute deviation between Corpsmembers and Center characteristics[b]				
• Sex	-0.015	60.569	-22.283****	-0.060
• Race/ethnicity	-0.093	-34.186	4.045	-0.014
• Location	-0.025	-4.999	0.070	0.014
Mean absolute deviation between Center and uniform distribution of characteristics				
• Sex	-0.609	-189.20***	-0.708	-1.093**
• Race/ethnicity	-0.394	-113.31	7.418	0.945*
• Location	0.165	33.150	-0.644	-0.718***
Percent of Corpsmembers in centers enrolled in GED program	-0.296	-84.807**	1.589	-0.387
Percent of Corpsmembers in centers enrolled in Occupational Training Cluster[c]				
• Sub professional	-2.175*	-255.91	-56.943****	-0.263
• Clerical and sales	0.320	131.51*	-11.066*	0.609
• Service occupations	0.256	61.332	1.290	1.894****
• Forestry, farming and gardening	1.394	138.49	1.489	3.159***
• Food service	0.661	29.438	-3.022	-2.141***
• Automotive and machinery repair	-0.495	-43.784	28.179***	1.683**
• Electrical/appliance repair	2.171	183.93	29.913	-2.213
• Industrial production	0.183	-29.014	6.259	0.555
• Transportation	0.595	118.48	-24.195**	2.548***
• Health occupations	-0.268	21.770	-16.640****	0.504
• Unassigned	1.528	209.80	-42.747**	-7.185****
• (F-value for all training variables)	(1.394)*	(1.612)**	(2.732)****	(3.312)****

*Significantly different from zero at the 80% level of statistical confidence (90% for a one-tail test).
**Significantly different from zero at the 90% level of statistical confidence (95% for a one-tail test).
***Significantly different from zero at the 95% level of statistical confidence (97.5% for a one-tail test).
****Significantly different from zero at the 99% level of statistical confidence (99.5% for a one-tail test).

[a] The findings presented in this table are based on multiple regressions; therefore, the estimates for each subgroup are net of the independent influences of all other subgroups in the table (i.e., we control for all of the variables in the table simultaneously). The estimates for all relevant subgroups would have to be averaged in order to obtain the estimate for a particular set of Corpsmember and center characteristics. The F-statistics shown in parentheses at the bottom of each factor (subset) are for null hypotheses that all of the regression coefficients underlying the results shown for each factor are simultaneously equal to zero.

[b] The mean absolute deviation is formally defined as $\frac{1}{n}\left|\sum_{i=1}^{n} X_{ai} - X_{di}\right|$, where n is the number of cells, X_{ai} is the actual value of X for the ith observation, and X_{di} is some designated level of X for that observation.

[c] The Construction Trades occupational cluster is the omitted category. The differential impacts estimated for all other clusters are deviations from the impact for the Construction Trades cluster.

The variables indicating the similarity between the characteristics of the center attended and a uniform distribution of those same characteristics more consistently affect postprogram labor-force activities and the length of stay in Job Corps. Sex deviation from a uniform distribution affects all of the outcome measures for males, and earnings and program completion for females. All of the effects are negative, which suggests that as centers move toward a uniform sex distribution of 50 percent males and 50 percent females, Corpsmember performance rises. Racial/ethnic deviations affect the subsequent earnings of males and program completion of females, but in opposite directions. Location deviations also sometimes appear to affect the program completion of females, but the pattern is unclear.

The distribution of Corpsmembers across occupational training clusters also tends to affect the various outcome measures. The estimated effects presented in the tables are actually deviations from Construction Trades training. Few if any generalizations can be drawn from these effects at this time because of the severe selectivity problems for interpretation.

D. SUMMARY AND CONCLUSIONS

Differential impacts among Corpsmembers and centers have been found along several important dimensions. The most basic differentials are associated with sex, family responsibility, and program completion categories. Relatively larger impacts for males are found for receipt of Unemployment Insurance and probability of being in military service, while relatively larger impacts for females without children are associated with employment and earnings, receipt of welfare, and education. The estimated Job Corps impacts for females with children are generally much smaller than for the other two groups.

A strong positive correlation exists between our estimates of Job Corps impacts and the amount of the Job Corps program completed. Program completers consistently benefit the most from the program, particularly in terms of employment, earnings, and dependence on welfare. Partial completers benefit less, and early dropouts little or not at all. Furthermore, these differential effects by completion categories seem to be at least partially attributable to the effect of staying in the program longer and completing, which indicates the potential for additional benefits to the program from increasing the length of stay and completions of Corpsmembers.

In an analysis of other differential impacts on employment and earnings, as well as on length of stay in Job Corps and program completion, we reconfirmed our earlier findings of differential outcomes associated with race/ethnicity and previous educational attainment. With the Job Corps' MIS data, we were also able to evaluate the differential impacts of several previously untested center-related variables. Completing a GED program is positively associated with the beneficial impacts, and, most importantly, the magnitude of this observed relationship is approximately the same as that for receiving a regular high school diploma. The occupational training received was also associated with differential impacts. Finally, more general center characteristics also seem to be associated with differential impacts. Examples include deviations of the center population from a uniform sexual composition and the Corpsmember distribution across occupational training clusters. The former is particularly interesting because the patterns of effects suggests that the attainment of the Job Corps' goal of 50 percent female participation might increase program completions and subsequent employment and earnings.

VII. ANALYSIS OF PROGRAM EXPENDITURES

This chapter examines the expenditures Job Corps used to operate the program during fiscal year 1977.[1/] We first present an overview of program expenditures, and then provide an assessment of the determinants of these expenditures. In addition, we present cost estimates and other information on program operating expenditures that, because they constitute the program component most directly controlled by DOL officials, are especially important for policy purposes.

It should be emphasized, however, that an analysis of program operating expenditures presents only half the picture. Expenditures can be changed by altering program operations (e.g., the mix of services or the types of training provided), but such changes may lead to corresponding changes in outcomes. Thus, while there are conclusions to be drawn from this expenditures analysis, overall evaluative assessments are better made in the context of impact and benefit-cost estimates and should not be based solely on the findings of this chapter.

A. OVERVIEW OF JOB CORPS OPERATING EXPENDITURES

Most data needed for the analysis were obtained from the Job Corps Financial Reporting System and pertain to fiscal year 1977. DOL defines the accounts and procedures for the system, and requires that they be used by all centers that file a Center Financial Report on a periodic basis. This report summarizes all expenditures incurred by the centers, as well

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[1/] This chapter summarizes a more complete analysis of expenditures presented in Technical Report J. For an analysis of those costs of Job Corps that are not included in the Job Corps budget, see Technical Reports F and K.

as the number of Corpsmembers served. Expenditures are reported on an accrual basis so that expenditure and enrollment data are comparable.[1/] The enrollment data used in this chapter to compute average or unit costs are "Corpsmember years," computed from average participant levels for the entire fiscal year.

Two other sources of data were also used. Data on central operating costs were obtained from the U.S. Office of Management and Budget, and cover both centrally budgeted program functions (such as recruitment and placement) and central administration by DOL. Data on the average length of time that participants stayed in Job Corps were obtained from the national Job Corps office. The estimated average length of Corpsmember participation (in years) is used to convert expenditures per Corpsmember year to a per-participant basis.

1. Total Operating Expenditures

Total expenditures for fiscal year 1977 are summarized in Table VII.1. These expenditures fall into three categories: center budgeted expenses, cash allowances to Corpsmembers, and central operating costs.

Center budgeted expenses include all costs of program functions budgeted at the center level. As shown in Table VII.1, this was by far the largest budgeted category of program expenditures during fiscal year 1977. A total of approximately $149 million, or 66 percent of the total program operating cost, was spent at the centers. Of the total center budgeted

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[1/] There may be a problem in estimating depreciation, because center facilities and equipment are retired at the center's acquisition cost, a procedure that does not account for depreciation over the life of the facilities. The effort introduced by this procedure is probably fairly small for fiscal year 1977, since capital use in that year did not appear to involve an unusual amount of acquisition or retirement.

TABLE VII.1

TOTAL PROGRAM OPERATING EXPENDITURES FOR JOB CORPS IN FISCAL YEAR 1977, BY CATEGORY

Category	Total Expenditure Fiscal Year 1977	Expenditures Per Corpsmember Year[a]
Center Budgeted Expenses		
Enrollee Expenses	$32,500,335	$1,567
Staff Expenses	79,034,227	3,811
Center Operating Expenses	27,128,221	1,308
Work Project Expenses	5,165,278	249
Capital Expenditure	10,499,624	506
Income	(5,569,349)	(269)
Total	$148,758,336	$7,730
Cash Allowances		
Total	$20,126,115	$970
Central Operating Costs		
Federal Administration	$19,200,000	$926
Recruitment and Placement	13,200,000	637
Engineering Support	13,600,000	656
Enrollee Transportation	3,600,000	174
Union Training Contracts	5,200,000	251
Other Miscellaneous	2,000,000	96
Total	$56,800,000	$2,739
Total Program Operating Expenditures	$225,684,451	$10,883[b]

SOURCES: Job Corps Financial Reporting Systems and the U.S. Office of Management and Budget.

[a] Expenditures per Corpsmember year are calculated by dividing the overall expenditure figures by 20,738.2--the total Corpsmember years among all centers in fiscal year 1977.

[b] Detail may not sum to total due to rounding.

expenses, staff expenses accounted for over 50 percent of the cost, or $3,811 per Corpsmember year. Enrollee expenses accounted for an additional 22 percent of the total, which amounted to $32,500,335, or $1,567 per Corpsmember year. These enrollee expenses were generally for the residential and support services provided to Corpsmembers. The remaining categories include expenditures for center maintenance, work project materials and supplies, and capital improvements. Center income is derived from providing food, lodging, and products to staff and visitors, as well as from canteen and theatre revenues; this income partially offsets the other center expenditures.

Cash allowances paid to Corpsmembers both during the enrollment period and at termination are separately budgeted and issued by the Army Finance Center. After enrolling in the Job Corps program, Corpsmembers received a monthly living cash allowance of $30 to be used to cover personal expenses. This allowance was increased in $5 increments to a maximum of $50 per month in six months. Corpsmembers also received an allowance at the time of their termination to help ease their transition to the outside world. This allowance was $50 for each month of participation in Job Corps if the participant had remained in the program long enough. (All of the allowances and their computation formulas have recently been made more generous, but these increases occurred well after fiscal year 1977.)

For fiscal year 1977, a total of $20,126,115 (approximately 9 percent of overall program expenditures) was paid to Corpsmembers. Cash allowances per Corpsmember year amounted to $970, or $477 per participant.

Central operating costs--expenditures for the federal administration of Job Corps and for centrally provided program services--were estimated by the U.S. Office of Management and Budget. The major centrally budgeted expenditures are for recruitment and placement services provided by regional

DOL offices and private agencies, as well as for engineering support (for work projects and center construction). The total cost of central operations was $56.8 million during fiscal year 1977, which was approximately 25 percent of total program expenditures. This amount represents $2,739 per Corpsmember year.

2. Average Operating Expenditures

The Job Corps operating costs reviewed above are best analyzed in terms of the units of service supplied (such as Corpsmember years). Aggregate expenditures are largely a function of both program size (that is, number of Corpsmember years) and the makeup of specific program service components. Analyzing average costs (expenditures per Corpsmember year) will shed light on what determines program operating costs other than the years of service provided to Corpsmembers. It will then be possible to assess these other factors in the context of program size.

Although 61 centers were operating during fiscal year 1977, only 54 were included in the sampling frame used for the benefit-cost evaluation of Job Corps. In order to be consistent with that evaluation, our analysis of average center operating costs will be limited to these 54 centers.[1/] This also eliminates certain analytical problems, since some of the omitted centers were still in their start-up phase in 1977 or were providing substantially different training services than most Job Corps centers. The average

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[1/] The 54 centers comprise the bulk of the Job Corps program. Approximately 96 percent of total Corpsmember years of service were recorded at these centers, along with approximately 96 percent of the center budgeted costs.

center expenditure per Corpsmember year for the 54 centers was approximately $7,700 during fiscal year 1977.[1]

Before turning our attention to the program features that determine this average cost, it will be useful to consider the types of variation in average cost. Variation in cost across the 54 centers is substantial: four centers recorded an average expenditure of under $6,500 in fiscal year 1977, while six centers had an average expenditure of over $9,000. This variation is caused by differences in the type of services provided, in the type of training, and in the size of centers, and can conveniently be viewed by comparing the differences between CCCs and contract centers. CCCs are generally smaller, are more rurally situated, are predominantly all male and residential, tend to provide more training in construction, and undertake more construction work projects than contract centers. The larger, more urban contract centers are more likely to have co-educational facilities and a nonresidential participation option than CCCs, and provide more training in service skills such as health occupations, clerical work, repairs, and food services. The average CCC package of services involves center operating expenditures of $8,455 per Corpsmember year. The average contract center package costs $7,130 per Corpsmember year.

The distinction between CCCs and contract centers is useful but masks the underlying causes of average cost variation. As discussed in the next section (and in Technical Report J), the packages of services offered by the two types of centers have evolved over time and could possibly be shifted if conditions warrant.

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[1] Central operating costs and the Corpsmember cash allowances are not included in this analysis because appropriate data are not available. Cash allowances are governed by uniform regulations and vary primarily because of differences in length of stay. The central operating costs are largely independent of cross-center differences.

The two major causes of differences in average cost are center size and the relative emphasis a center places on Corpsmember training in the construction trades (see Technical Report J for more details). A comparison of different-sized centers shows that average expenditures per Corpsmember fall quickly as size increases (although such economies of scale seem to taper off once the scale exceeds 600 Corpsmember years of service per year). Center costs also decrease as the percentage of Corpsmembers who receive training in construction trades decreases. Centers with over half of their training programs in construction recorded costs per Corpsmember year that were over $1,200 higher than centers with less than 25 percent of training in the construction trades.

B. DETERMINANTS OF OPERATING EXPENDITURES

To determine how aspects of center operations influence costs, it is necessary to disaggregate costs along functional lines. These functional costs fall into four major expense groups: enrollee expenses, center operations expenses, staff expenses, and work-project expenses. The four types of expenses are shown in Table VII.2 and will be considered separately below.

1. Enrollee Expenses

As shown in Table VII.2, average enrollee expenses are more than $900 higher at CCCs than at contract centers, and CCC costs are higher for almost all enrollee expense categories. The differences appear to be caused by three main factors. First, average enrollee expenses are inversely related to center size (significant at the 99% level), which

TABLE VII.2

CENTER OPERATING EXPENDITURES PER CORPSMEMBER YEAR IN FISCAL YEAR 1977, BY CENTER TYPE

	Center Type		
Type of Expense	CCCs	Contract Centers	All Centers
Enrollee Expenses			
Residential	$1,199	$943	$1,071
Educational/Vocational	964	302	662
Recreation	105	78	92
Transportation	8	45	27
Total Enrollee Expenses	$2,276	$1,368	$1,822
Operations Expenses			
Maintenance	$264	$158	$212
Utilities, Fuel, Supplies	628	632	630
Other	0	748	374
Total Operations Expenses	$892	$1,538	$1,216
Staff Expenses			
Management and Support Staff	$1,396	$1,615	$1,506
Educational/Vocational Staff	661	853	759
Other Staff	1,835	1,320	1,578
Other Staff-related Expenses	84	38	61
Total Staff Expenses	$3,976	$3,826	$3,904
Work Project Expenses			
Supplies and Equipment-Operating Expenses	$903	$50	$477
Construction	465	380	423
Equipment	216	206	212
Total Work Project Expenses	$1,584	$636	$1,111
Total Center Operating Expenses	$8,728	$7,368	$8,053

SOURCE: Job Corps Financial Reporting System. The data on expenditures and Corpsmember years used to compute these figures are from the 54 centers in the evaluation sample.

suggests that there are economies of scale in delivering residential and educational/vocational services to enrollees.

Second, approximately half of the contract centers (but no CCCs) had nonresidential programs for Corpsmembers during 1977. This accounts for part of the over $250 difference between CCCs and contract centers for residential expenses, as well as for part of the difference for recreational expenses.

Third, there is a very substantial difference--almost $700 per Corpsmember year--in the amount of money that CCCs and contract centers spend on vocational services and supplies. Moreover, vocational service and supply costs vary markedly between centers within the CCC and contract-center groups; this variation far exceeds that associated with either residential or recreational expenses. The variation is closely related to the industry mix of vocational training provided by centers. In particular, the level of the expense is highly correlated with the ratio of construction training to all training programs offered by a center.

2. Operations Expenses

Table VII.2 shows that center operations expenses per Corpsmember year at contract centers were over $600 more than at CCCs. However, this aggregate difference is quite misleading because of accounting differences between CCCs and contract centers (see further below).

Average maintenance costs were over $100 higher at CCCs than at contract centers during fiscal year 1977. This expense is inversely related to center size (significant at the 95% level), again indicating economies of scale in this aspect of program operations. Utility, fuel, and supply expenses per Corpsmember year are very similar for CCCs and

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contract centers, and vary relatively little among centers within those two groups. However, other operations expenses (for legal and accounting services, insurance, and center security) were dramatically different at CCCs and contract centers: zero at the former, and almost $750 at the latter. The reason for this discrepancy is that CCCs, which are operated by the federal government, do not include these services as direct costs in their own budgets (they are an overhead expense in the central agency budget). This substantial difference more than accounts for the aggregate difference between the center operations expenses of the two types of centers.

3. Staff Expenses

Job Corps center staffs comprise management and support, educational and vocational, and other center personnel assigned to safety and recreation, guidance and counseling, medical and dental care, and the supervision of work projects. Table VII.2 shows a breakdown of staff compensation and related expenditures by staff category and center type. Staff expenses are greater than expenditures in the other three major expense categories.

Total staff compensation per Corpsmember year at CCCs exceeds contract center compensation by approximately 4 percent. Total staff cost is inversely related to center size (significant at the 99% level), which suggests potential economies of scale. However, several categories of staff compensation appear to be more closely related to factors other than center size. Most of these factors stem from institutional differences among Job Corps centers. For example, contract centers recorded compensation costs for vocational and educational personnel that were almost $200 higher per Corpsmember year than those of CCCs, due entirely to the contract centers' practice of utilizing more vocational personnel and less work project staff in providing training to Corpsmembers. The CCCs actually spent more on educational staff: $567 compared to $449. The contract centers do not rely heavily on work projects (nor, thus, on work project staff) in the training process. The CCCs spent $446 per Corpsmember year on work-project personnel, while contract centers spent only $37. This also reflects industrial differences in the types of training offered by centers.

Similarly, Table VII.2 indicates that contract centers had higher compensation costs for safety and recreation staff. This reflects both differences in center recreation and safety programs and in the way centers pay for them. Notably, CCCs spent more per Corpsmember year on contracted recreation services, which is part of the recreational cost reported in Table VII.2.

4. Work Project Expenses

Work projects are a central part of vocational skills training in the construction trades. The costs incurred by centers in these work projects fall into three categories: work-project supplies and equipment operation, center construction and rehabilitation, and capital equipment expenses. As indicated in Table VII.2, all three expenses are higher at CCCs than at contract centers, and the difference in total work-project costs is almost $1,000 per Corpsmember year.

Contract centers undertake few work projects that do not constitute capital improvements in center facilities. Thus, the expenditures recorded by contract centers for work-project supplies and equipment operation is very small--only $50 per Corpsmember year. Most contract centers have no expenses of this type, while a few do have work projects in the surrounding community. In contrast, CCCs devote over

$900 per Corpsmember year for supplies and equipment operation in community-serving work projects. This difference accounts for most of the overall CCC/contract-center disparity in work-project expenses. It reflects not only institutional differences between the two types of centers in the use of work projects to provide on-the-job training, but also the greater emphasis that CCCs place on training in the construction trades.

Both contract centers and CCCs have construction projects designed as center capital improvements, such as dormitory construction and facilities rehabilitation. The difference between what the two types of centers spend on these projects is less than $100. In addition, the amounts spent by CCCs and contract centers on capital equipment acquisitions (equipment used in work projects and for other purposes) is very similar.

C. SUMMARY AND CONCLUSIONS

The determinants of Job Corps operating costs have been assessed for each of the three major categories of costs: center operating expenses, cash allowances, and central operating expenses.1/ We concluded that center operating expenses are determined largely by center size, the industrial mix of vocational training offered, and institutional factors associated with center administration (i.e., CCCs versus contract centers). The importance of other factors, such as the geographic location and co-educational status of centers, is more difficult to identify. Cash allowance expenses are primarily a function of the uniform allowance rules and the average length of Corpsmember stay in the program. Central operating costs primarily reflect federally budgeted administrative functions and contracts for recruitment and placement, which, for the

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1/See Technical Report J for a more detailed discussion of the latter two categories.

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most part, are determined independently from center operations and from support of construction training and work projects.

Three observations should be made about these general findings. First, the results of the analysis are hardly surprising. Job Corps is an established social program. Consequently, what had been budgeted for individual centers in fiscal year 1977, as well as for various program components, had been worked out over a period of years. CCCs have always been the rural, smaller centers providing more of the program's training in the construction trades. Contract centers have long been the larger, urban, service-training centers. Moreover, major structural changes in Job Corps have not occurred since the late sixties,[1] and, as noted at the outset of this report, the recent expansion of the program began after fiscal year 1977. Therefore, as is clear in Table VII.3, average center costs (in constant dollars) were relatively stable over the 1970-77 period, as were the relative expenditures for both CCCs and contract centers. This stability also applies to most of the categories of operating costs discussed in previous chapters of this report. Clearly, the patterns in Job Corps expenditures that have emerged in this analysis represent, to a great extent, budgetary formulas that were developed over several years of program operations.

Second, this cost analysis has been limited by the realities of Job Corps operations. The determinants of program operating costs (center size, location, industrial types of training, use of work projects, co-educational status, nonresidential enrollment, and so on) are interrelated, which makes it difficult to estimate the unique importance of

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[1] During the late sixties, responsibility for the program was shifted from OEO to the Department of Labor, enrollment of women was increased after a legislative mandate, and 59 centers were closed.

TABLE VII.3

JOB CORPS OPERATING COSTS PER CORPSMEMBER YEAR, FISCAL YEARS 1970-77

	Fiscal Years							
	1970	1971	1972	1973	1974	1975	1976	1977
Total Expenditures[a/] (millions of dollars)	$ 267	$ 264	$ 278	$ 242	$ 214	$ 191	$ 199	$ 231
Corpsmember Years	20,840	22,394	23,808	23,126	21,287	20,707	21,119	21,615
Total Expenditure per Corpsmember Year	12,800	11,800	11,670	10,480	10,040	9,200	9,430	10,687
Center Expenses and Cash Allowances per Corpsmember Year:								
CCCs	-	-	9,437	-	-	-	-	9,434
Contract Centers	-	-	8,980	-	-	-	-	8,109

SOURCES: *Job Corps in Brief* (Fiscal Year 1977), U.S. Department of Labor, Employment and Training Administration; and the Job Corps Financial Reporting System.

[a/] This is the total applied funding for Job Corps adjusted by the GNP price deflator to reflect 1977 dollars.

any one variable. Also, given the stability mentioned above, particularly with respect to the fact that center size and expenditures vary little over time, it is difficult to estimate the magnitude of scale economies and the marginal costs of serving Corpsmembers. There is certainly evidence that large centers have lower average costs than small centers, but it is difficult to isolate precisely the role of center size in determining costs based on 1977 data. Program expenditure data for fiscal years 1978-80 could provide a basis for analyzing the marginal costs of Job Corps.

Third, it should be emphasized that any conclusions regarding Job Corps operating costs should ideally be interpreted in a broad program context. Job Corps expenditures reflect most, but not all, resources used by the program; other resources are paid for by other agency budgets and by some nongovernmental organizations. In addition, many of the direct and indirect benefits of Job Corps constitute "offsets" to other agency budgets. For example, reduced income maintenance payments, welfare agency administrative costs, and criminal justice system costs associated with participation in Job Corps--as well as output from work projects and work-experience programs that benefits government agencies--all could be viewed as offsets in estimating a "net cost" of the program to all government budgets.

Finally, program costs should not be viewed independently of the program services that are provided and the benefits that result. This is especially important insofar as cost comparisons are concerned. This applies to comparisons between Job Corps centers that may not provide identical services, notably in terms of industrial types of training or residential services supplied. It also applies to comparisons of Job Corps costs to those of other employment and training programs, many of which are not residential programs, pay wages instead of small allowances, or differ in other important ways from Job Corps.

VIII. GENERALIZABILITY OF THE FINDINGS

One goal of this evaluation was to provide a general statement about the overall effectiveness of Job Corps as an employment and training program for disadvantaged youths. The statement was to be made on the basis of differences found between a random sample of Corpsmembers and a carefully selected comparison group. However, a number of questions arise which could possibly inhibit our ability to generalize the findings from the evaluation sample to the Job Corps population as it was in 1977 when the sample was drawn, as well as to potential Job Corps populations of the future:

> Is our sample of Corpsmembers similar to the Job Corps population in 1977, to later Job Corps populations and to more general populations of disadvantaged youths that may be of interest (at least similar enough to facilitate correct statistical inferences)?
>
> Has the Job Corps program changed in any major substantive ways since 1977?
>
> Has the social and economic background against which the Job Corps evaluation sample was observed changed in any ways that are likely to influence our estimates of the effectiveness of Job Corps?
>
> To what degree are our estimates biased by our having used a comparison sample rather than a "true" control group, or by other peculiarities of the analytical approach?
>
> And, finally, what is the overall quality of the data used in the evaluation?

Each of these questions will be discussed in turn in the remainder of this chapter.

A. REPRESENTATIVENESS OF THE SAMPLE AND CHANGES IN THE CHARACTERISTICS OF CORPSMEMBERS SINCE 1977

In Table VIII.1 we compare the percentage distributions for several demographic characteristics of our Job Corps evaluation sample to those for the Job Corps populations of 1977 and 1979. We found that the evaluation sample is, on average, similar to both populations, in terms of sex, age, race/ethnicity, and educational background.

It is reasonable to question further whether future generations of Corpsmembers will be affected by Job Corps participation in ways similar to our evaluation sample--that is, can we expect future enrollees to exhibit increased employability and earnings, and to derive the other benefits that were found for the evaluation sample? Because our sample showed no evidence of contradicting the underlying theory of the traditional economic models of behavior tested in our analysis, we have no reason to believe that the behavior of future Job Corps participants will contradict this theory and thus be affected by the program differently than our evaluation sample. (Of course, it is unlikely that the exact estimates of Job Corps' effectiveness would be replicated.)

Since 1977 the Job Corps program has begun to double in size--from 22,000 program slots to 44,000. This has been done by expanding the program in high-poverty, high-unemployment areas and particularly by encouraging the enrollment of women (especially those with children), Hispanics and American Indians, handicapped individuals, and other targeted groups. As shown in Table VIII.1, however, the overall demographic composition of Job Corps changed very little between fiscal years 1977 and 1979. The education and training component of the program has been enhanced with the expansion of Job Corps to include more positions in junior colleges

TABLE VIII.1

COMPARISONS OF EVALUATION SAMPLE TO JOB CORPS POPULATIONS IN 1977 AND 1979

	Evaluation Sample 1977[a]	Job Corps Population 1977	Job Corps Population 1979
Sex			
• Percentage male	73	69	70
• Percentage female	27	31	30
Age			
• Percentage under age 18	49	49	48
• Average age in years	18	18	18
Race			
• Percentage white (non-Spanish-speaking including Asians and Pacific Islanders	25	31	33
• Percentage Black	58	54	53
• Percentage Spanish-speaking	11	11	10
• Percentage American Indian	6	4	4
Education			
• Percentage with fewer than 12 years	88	85	86
• Average number of years	10	10	10

[a] Reweighted by Job Corps completion categories to be representative of all Job Corps enrollees.

and specialized preparation for military service for individuals who had previously failed to meet military admissions requirements. In addition, greater ties have been sought between Job Corps and other employment programs such as CETA and WIN. It would be difficult to imagine any of these changes having a negative effect on the ability of Job Corps to improve employment opportunities for disadvantaged youths. In sum, the essence of the Job Corps program has not changed since 1977.

As to the social and economic context against which the program was set, the employment rate for youths in the regular sector (i.e., aside from special government programs) has not improved since the sample period, may have gotten worse, and does not look promising for the near future. Economic fluctuations no doubt affect Job Corps impacts. However, to some extent, this is a separate issue and one over which Job Corps exercises no control. Fiscal year 1977 was relatively typical of the recent labor experiences for disadvantaged youths.

Furthermore, there are now many more alternative employment programs for disadvantaged young people than there were in 1977. However, our analysis has shown that Job Corps had a larger impact on earnings than other training programs available to our sample and was an efficient social investment. (See Chapter VIII for a comparison of Job Corps with other programs, and Chapter V for results of the benefit-cost analysis.) We conclude that, although the Job Corps program has continued to grow and change and the economy and general plight of disadvantaged youths may have worsened, our estimates of the benefits of Job Corps participation can be generalized at least in broad implications to potential future generations of Corpsmembers and may, in fact, be somewhat understated for them due to program improvements.

B. THE VALIDITY OF THE COMPARISON SAMPLE STRATEGY

In a perfectly controlled experiment, individuals are randomly assigned either to a group that receives a treatment or to a control group that receives no treatment. This randomization ensures that differences between the two groups measured after treatment can be attributed only to the treatment and not to unobserved differences between the groups. However, randomization was not possible for the evaluation of an ongoing program in the Job Corps context. Therefore, instead of having a true control group, we selected a comparison sample that matched our participant sample as closely as possible.

Comparison-group members were selected with a two-stage process in which fifteen areas of the country were chosen in the first stage because they were similar to the areas from which Corpsmembers came, but in which Job Corps did not recruit extensively. One hundred youths at each site were then selected in the second stage from school dropout and employment service lists, so that 70 percent were young, recent dropouts, and 30 percent were older dropouts who had been out of school longer (the assigned selection probabilities ensured comparability in terms of education levels, age, race/ethnicity, etc.).

Regression analysis was used to control for differences between the participant and comparison samples with respect to various demographic and socioeconomic characteristics, such as age, sex, race/ethnicity, education, prior health, prior drug use, criminal history, and prior employment. Other econometric techniques were used to control for unmeasured preprogram traits, such as motivation and innate employability. (See Chapter III for a more detailed discussion of the econometric methodology employed in the evaluation.)

Even though we were able to control for inherent differences between participants and comparisons with respect to both measured and unmeasured preprogram characteristics, we had to make a second departure from the controlled experimental design. While participants were receiving the Job Corps "treatment," comparison-group members were not treatment-free--there were many other employment and training programs potentially available to them. However, as mentioned earlier, we found that participation in other training programs was low, and that Job Corps had a significantly larger impact on earnings than other training programs available to our sample. Therefore, we conclude that the comparison sample provided an adequate standard against which the effectiveness of Job Corps could be measured.

C. DATA QUALITY

When analysis data are pulled from survey interviews, data quality is always a twofold question. First, are the results biased by our inability to interview certain individuals either because they could not be located or refused to be interviewed? Second, given that we are able to interview an individual, how accurate are the responses we record for him or her?

An investigation into the problems associated with nonresponse to Job Corps evaluation interviews showed that overall response rates were relatively high. On average, approximately 85 percent of the sample responded to at least one follow-up interview and were thus available for the analysis of postprogram behavior. Even though the response rate for Corpsmembers was approximately 15 percentage points lower than it was for comparisons, we did not find that nonresponse had biased estimates of the impact of Job Corps on employment, earnings, or frequency of arrest.

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(See Technical Report L, "An Analysis of Nonresponse to Job Corps Evaluation Interviews," for the details of this investigation.) If anything, nonresponse adjustments tend to show slightly larger impacts than we have presented.

It should also be noted that the second follow-up interview was administered to some individuals in person and to some by telephone, while all previous interviews were administered in person. Due to budget restrictions, part of the sample eligible for a second follow-up interview was contacted by telephone only, while the remainder were subject to an in-person search if telephone contact could not be made. Previous studies which compared the quality of data from telephone interviews with those from in-person interviews have found that differences in the willingness of individuals to submit to each type of interview are minimal, and that the different interview modes do not appear to affect data quality to any great extent (see Groves, 1977; Rogers, 1976; Colombotos, 1969).

Not surprisingly, we found that individuals who were eligible for an in-person contact were, on average, approximately 9 percentage points more likely to respond than those who were eligible for a telephone contact only (see Technical Report L). However, as mentioned above, nonresponse does not seem to have biased estimates of program impacts. Furthermore, item nonresponse was very low (almost nonexistent, except for some recall problems at baseline). With the second follow-up interviews, for example, despite both the large number of questions and the existence of several data items serving only as interviewer checks, less than one data item per interview on average was not complete.

D. SUMMARY AND CONCLUSIONS

While the exact estimates and single numbers do not generalize very well, we are relatively confident in the broad implications of our findings for disadvantaged youths and for Job Corps in particular. The evaluation has been largely successful in its narrow range of objectives (including development of innovative procedures for comparison-group metholodogies). Furthermore, useful data have been provided for additional research on the difficult employment problems faced by disadvantaged youths.

IX. SUMMARY AND CONCLUSIONS

This second follow-up report has presented the main findings from a study designed to provide the Department of Labor with comprehensive evaluations of both the short-term economic impact of the Job Corps program for participants and the benefits and costs of the program.[1] The information provided herein is based on the most comprehensive data yet available to conduct a study of Corpsmembers. Comprehensive interviews were first conducted in the spring of 1977 with a sample of Corpsmembers who were then participating in the program and with a comparable group of disadvantaged youths who had not attempted to enroll in Job Corps. At periods 9 and 24 months after the baseline survey, reinterviews were conducted with all of the youths in the comparison group and with Corpsmembers who had been out of the program for a long enough time to provide useful postprogram information.

The baseline survey obtained detailed information on the demographic characteristics of the youths, their social-economic backgrounds, and their work histories and related activities beginning six months before the Corpsmembers enrolled in the program and continuing up to the date of the interview, which represented approximately six months of program experience. The two follow-up surveys continued to collect detailed information on work histories and related activities during the postprogram period when Corpsmembers had been out of the program from one to two years, with an average of slightly over 18 months. Altogether, the data base for this evaluation has both baseline and follow-up data on approximately 5,100 youths.

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[1] Also available from this evaluation are fourteen other reports that document specific topics in more detail (for more details, see the Contents page and Chapter I).

The findings on Corpsmembers' postprogram behavior are generally consistent with the hypothesized economic impacts and the important program goal of improving participants' economic prospects. During the first two postprogram years we find that Job Corps is at least moderately successful overall in achieving its desired effect of (1) increasing employment and earnings, (2) improving future labor-market opportunities, work experience, education, training, health, geographic mobility, and military service, (3) reducing dependence on welfare assistance and other public transfers, and (4) reducing criminality.

Some of the most noteworthy findings of Job Corps effects on the behavior of former participants during their second postprogram year can be summarized as follows (on a per Corpsmember basis): (1) an increase in employment of over 4 weeks per year, (2) an increase in earnings of approximately $500 per year, (3) a 4 percentage-point increase in military service, up from 5 percent to 9 percent, (4) an increase in the probability of having a high school diploma or equivalent degree, from 11 percent to 36 percent, (5) higher college attendance, equivalent to an increase of nearly 5 full-time college students for every 100 youths enrolled in Job Corps, (6) a reduction in the receipt of financial welfare assistance, amounting to nearly 3 weeks per year, and (7) a reduction in the receipt of Unemployment Insurance of over one week per year.

The positive, overall impacts generally persist through the second year of postprogram observation. The trend over the two-year postprogram observation period appears to be an increase in program benefits during the first few months (especially for employability during the transition from center life to re-entering the regular labor market), and then relatively stable effects through the rest of the two-year period. The one exception is for criminality, which shows relatively large reductions in the very early postprogram period that fade out rapidly after Corpsmembers are out of the program for a year. For employment and earnings we find very stable estimates of gains among civilians, especially program completers, for months 6 to 20 in the postprogram period but, in fact, substantial growth in program effects on employment and earnings when the increasing military gains are incorporated.

Differential impacts among Corpsmembers are found to be associated with sex, family responsibility, and program-completion categories. Relatively larger impacts for males are found for the receipt of Unemployment Insurance and for the probability of being in military service, while relatively larger impacts for females without children are found for civilian employment and earnings, the receipt of welfare, and education. The estimated Job Corps impacts for females with children are generally much smaller than for either males or females without children. A substantial, positive correlation is found between the estimated Job Corps impacts and the proportion of the Job Corps program completed. Program completers consistently benefit the most, particularly in terms of employment, earnings, and dependence on welfare. Partial completers are found to benefit less, and early dropouts are found to benefit little or not at all. Furthermore, these differential impacts by completion category seem to be at least partially attributable to the effect of staying in the program longer and completing the program, which indicates the potential for additional benefits to the program from increasing the length of stay and completions of Corpsmembers.

We find additional differential impacts associated with program and center-related variables, although the causality of these differences cannot be attributed with any degree of accuracy. Completing a GED program is positively associated with the beneficial impacts, and, most importantly, the magnitude of this observed relationship is approximately the same as that for receiving a regular high school diploma. Differential impacts are also found to be associated with the industrial type of vocational training received and the characteristics of the centers that Corpsmembers attended (such as the sexual composition, which suggests that a more equal sexual composition would increase program completions and subsequent employment and earnings).

In an exploratory analysis we find significant Job Corps effects in terms of reducing extramarital children and delaying family formation for females. Furthermore, our estimates of overall Job Corps impacts are probably biased downward by not incorporating either the Job Corps effects on family composition or the employment and related effects for females with children.

The findings from a comprehensive evaluation of the social benefits and costs of Job Corps suggest that public investment in Job Corps is economically efficient. Our benchmark estimate is that the value of benefits in fiscal year 1977 exceeded costs by almost $2,000 per Corpsmember, or by approximately 39 percent of costs. Furthermore, the program is found to be economically efficient under a wide range of alternative assumptions and estimates. Because over 40,000 youths enrolled in Job Corps during fiscal year 1977, our benchmark estimate of the net social benefit for the entire program is approximately $80 million for that year.

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We estimate that over 60 percent of the social benefits come from increases in the value of output that Corpsmembers produced. Another 30 percent of the social benefits are attributable to reductions in criminal activity among Corpsmembers, particularly burglary and larceny.

In assessing the distribution of benefits and costs we find a net transfer from non-Corpsmembers as a group (everyone in society other than Corpsmembers) to Corpsmembers. The main economic benefits to Corpsmembers are derived from increased earnings (approximately 70 percent of the benefits) and transfers received while they are in Job Corps. The main economic benefits to non-Corpsmembers are derived from reductions in Corpsmembers' criminal activities and in their use of transfer programs.

Sensitivity tests were undertaken for a wide variety of the assumptions and estimates that are used in the benefit-cost analysis, and these sensitivity tests generally confirm that Job Corps is an economically efficient program. With respect to the critical parameter of future growth or fadeout of effects, we find that Job Corps is an economically efficient use of resources as long as the earnings effects do not decay more rapidly than 37 percent per year after our observation period.

We find that center operating expenses per Corpsmember are determined largely by center size, the industrial mix of vocational training offered, and institutional factors associated with center administration (i.e., CCCs versus contract centers). Other factors such as the geographic location and coeducational status of centers appear to be somewhat important but are more difficult to isolate. The largest differences in operating expenses are associated with scale economies of larger center size up to approximately 600 Corpsmember positions at a center.

While the estimates are not exact and single numbers do not generalize very well, after a careful analysis we are relatively confident about the broad implications of our findings for disadvantaged youths in general and for Job Corps in particular. The most tentative finding concerns whether the effects observed in the second postprogram period will fade out, grow, or remain stable in the future. Only further postprogram observation can adequately answer that issue.

In the report we presented detailed discussions of all of the findings summarized above. In addition, useful data are available from this evaluation for further research both on the effects of Job Corps and on the difficult problems faced by disadvantaged youths in general.

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